

# MIND

## A QUARTERLY REVIEW

### OF

## PSYCHOLOGY AND PHILOSOPHY

### I.—WITTGENSTEIN'S LECTURES IN 1930-33

BY G. E. MOORE

#### II

( $\beta$ ) THE third kind of "proposition" mentioned in Part I<sup>1</sup> (p. 10), of which at the very beginning of (I) Wittgenstein gave mathematical propositions as an example, saying that they are a "very different sort of instrument" from, *e.g.* "There is a piece of chalk here", and of which he sometimes said that they are not propositions at all, were those which have been traditionally called "necessary", as opposed to "contingent". They are propositions of which the negation would be said to be, not merely false, but "impossible", "unimaginable", "unthinkable" (expressions which he himself often used in speaking of them). They include not only the propositions of pure Mathematics, but also those of Deductive Logic, certain propositions which would usually be said to be propositions about colours, and an immense number of others.

Of these propositions he undoubtedly held that, unlike "experiential" propositions, they cannot be "compared with reality", and do not "either agree or disagree" with it. But I think the most important thing he said about them, and certainly one of the most important things he said anywhere in these lectures, was an attempt to explain exactly how they differed from experiential propositions. And this attempt, so far as I can see, consisted in maintaining with regard to them two things, *viz.* ( $\beta'$ ) that the sentences, which would commonly be said to express them, do in fact, when used in this way, "say nothing" or "are without sense", and ( $\beta''$ ) that this supposed fact that

<sup>1</sup> MIND, January 1954, No. 249.

such sentences, when so used, are without sense, is due to the fact that they are related in a certain way to "rules of grammar". But *what*, precisely, was the relation to grammatical rules, which he held to be the reason why they had no sense? This question still puzzles me extremely.

For a time I thought (though I felt that this was doubtful) that he held so-called necessary propositions to be *identical* with certain grammatical rules—a view which would have yielded the conclusion that sentences, which would commonly be said to express necessary propositions, are in fact always merely expressing rules of grammar. And I think he did in fact hold that the very same expressions, which would commonly be said to express necessary propositions, can also be properly used in such a way that, when so used, they merely express rules of grammar. But I think he must have been aware (though I think he never expressly pointed this out) that, if so, then, *when* such expressions are being used merely to express rules of grammar they are being used in a very different way from that in which, on his view, they are being used when they would commonly be said to be expressing necessary propositions. For he certainly held, if I am not mistaken, of *all* expressions which would commonly be said to be expressing necessary propositions, what in the *Tractatus* he had asserted to be true of the particular case of "tautologies", viz. both (1) that, when so used, they are "without sense" and "say nothing", and (2) that, nevertheless, they are, in a certain sense "true", though he made plain, in these lectures, that he thought that the sense in which they are "true" was very different from that in which experiential propositions may be "true". (As I have said (p. 11), he seemed to me often to use the words "proposition" and "sentence" as if they meant the same, perhaps partly because the German word "Satz" may be properly used for either; and therefore often talked as if sentences could be "true".) But of the same expressions, when used, as he thought they might be, merely to express rules, though he might perhaps have said that they "say nothing", since he insisted strongly of one particular class of them, namely, those which express rules of deduction, that they are neither true nor false, he cannot, I think, have held that they are "without sense"; indeed he said, at least once, of an expression which would commonly be said to express a necessary proposition, "if it is to have any meaning, it must be a mere rule of a game"—thus implying that, if it is used to express a rule, it has a meaning. But in what sense was he using "rules", when he insisted that his own "rules of inference" were neither true

nor false? I think this is an important question, because he seems to me to have used the expression "rules of grammar" in two different senses, the difference between which he never expressly pointed out, and one of which is such that a grammatical rule, in that sense, will be true or false. He often spoke as if rules of grammar *allowed* you to use certain expressions and *forbade* you to use others, and he gave me the impression that, when so speaking, he was giving the name of "rules" to actual statements that you are allowed or forbidden to use certain expressions—that, for instance, he would have called the statement, "You can't say 'Two men *was* working in that field'" a rule of English grammar. This use of "can't" is, indeed, one which is quite natural and familiar in the case of rules of games, to which he constantly compared rules of grammar; e.g. a chess-player might quite naturally say to an opponent, who was a beginner and was not yet familiar with the rules of chess, "You can't do that" or "You can't make that move", if the beginner moved a pawn, from its position at the beginning of the game, three squares forward instead of only two. But, if we so use "rule" that the expression "You can't do that", when thus used, is expressing a rule, then surely a rule *can* be true or false; for it is possible to be mistaken as to whether you can or can't make a certain move at chess, and "You can't do that" will be true, if it is an *established* rule in chess not to make the kind of move in question, and will be false if there is no such established rule. But if we ask: What is the rule which is established in such a case? we come upon a very different sense of "rule"; for the answer to this question will consist in describing or specifying a way in which somebody *might* act, whether anybody ever does so act or not; and with this sense of "rule" it seems to me obvious that a rule cannot be true or false, and equally obvious that any expression which specifies it will have sense. In the case of rules of grammar, the possible action which such a rule specifies, will, of course, be a way of using words or forms of sentence in speaking or writing; and I think that the fact that "rule" may be used in this sense, in which a "rule" can obviously be neither true nor false, may have been partly responsible for Wittgenstein's assertion that his "rules of inference" were neither true nor false. It is perhaps worth noting that the statement that such a rule is an *established* rule in a given language, (as is implied for English by, e.g., the statement, "You can't say 'Two men *was* working in that field'"), which really is true or false, is, of course, an experiential proposition about the way in which words or forms of sentence are actually used in the

language in question ; and that, therefore, if we suppose that the very same expression which is sometimes used to express a necessary proposition can also be used to express such an experiential proposition, then the ways in which it is used in these two cases must be very different ; just as the ways in which the same expression is used, if used sometimes to express a necessary proposition and sometimes merely to specify a possible way of speaking or writing, must also be very different.

I think, therefore, that Wittgenstein cannot possibly have held that expressions which are being used in the way in which they would commonly be said to be expressing necessary propositions, are being used in the same way in which they are being used when used to express rules of grammar. But, if so, to *what* relation to rules of grammar did he hold it was due that expressions which are being used in the former way, have no sense ? I am still extremely puzzled as to the answer to this question, for the following reason.

He seemed often to suggest that any sentence which is "constructed in accordance with" (this is his own phrase) the rules of grammar of the language to which the sentence belongs, always has sense ; *e.g.* that any English sentence which is constructed in accordance with the rules of English grammar, has sense. But, if so, since he held that, *e.g.* the sentences " $2 + 2 = 4$ " or "The proposition with regard to any two propositions that they are not both false follows logically from the proposition that they are both true", both of which would certainly be commonly said to express necessary propositions, are, when so used, without sense, he must have held that these two sentences, when so used, are not constructed in accordance with the rules of English grammar. Can he possibly have held that they are not ? I think it is possible he did ; but I do not know. But in the passage which I have already quoted (p. 13) about Helmholtz's statement that he could imagine the fourth dimension, he seemed to be saying that if Helmholtz was "projecting" the sentence "I can imagine a piece of chalk being thrown into the fourth dimension" "with the common method of projection", then he was talking nonsense, but that if he had been "projecting" that sentence in an unusual way, so that it meant the same as "I can imagine a piece of chalk first disappearing and then appearing again", then he would have been talking sense. But is not "projecting with the common method of projection" merely a metaphorical way of saying "using in accordance with the established rules of grammar" ? If so, then Wittgenstein was here saying that a sentence used in accordance with the



established rules of grammar may nevertheless *not* make sense, and even implying that, in particular cases, the fact that it does not make sense is (partly) due to the fact that it *is* being used in accordance with the usual rules. I think, however, that possibly he intended to distinguish between "projecting with the common method" and "using in accordance with the usual rules", since he insisted strongly in at least one passage that any rule can be "interpreted" in different ways, and also (if I have not misunderstood him) that it is impossible to add to any rule an unambiguous rule as to how it is to be interpreted. Possibly, therefore, he meant by "projecting with the common method", *not* "using in accordance with the usual rules", but "*interpreting* in the usual manner"—a distinction which would apparently allow him to hold that, when Helmholtz uttered his nonsensical sentence, he was *not* using that sentence in accordance with the usual rules, though he *was* interpreting in the usual manner the rules, whatever they may have been, in accordance with which he was using it. But I am very puzzled as to how this distinction could be used. Suppose, for instance, a person were to use "I can imagine a piece of chalk being thrown into the fourth dimension" in such a way that it meant the same as "I can imagine a piece of chalk first disappearing and then appearing again", how on earth could anyone (including the person in question) possibly decide whether in such a case the speaker or writer was doing what Wittgenstein called elsewhere "changing his grammar", *i.e.* using the first expression *not* in accordance with the usual rules, but in accordance with rules such that it meant the same as the second means, or whether he was merely "interpreting" in an unusual way the rules, whatever they may have been, in accordance with which he was using the first expression? I suspect, therefore, that when Wittgenstein said that Helmholtz must have been using the "common method of projection", when he uttered his nonsensical sentence, he was not distinguishing this from using the sentence in accordance with the ordinary rules, and was therefore implying that a sentence constructed in accordance with the ordinary rules might nevertheless be without sense. But, if so, his view may have been that, *e.g.* " $2 + 2 = 4$ ", when used in the way in which it would commonly be said to express a necessary proposition, *is* used in accordance with the ordinary rules of grammar, and is nevertheless "without sense", and is so partly *because* it *is* used in accordance with the ordinary rules; for he certainly would not have denied that that expression *might* be used in such a way that it had sense. But I do not know whether this was his view or not.

But finally there is still another reason why I am puzzled as to what his view was about sentences, which would commonly be said to express necessary propositions. His view was, if I am right, one which he expressed by the use of the expressions, ( $\beta'$ ) "without sense", as equivalent to which he often used the expressions "nonsense", "meaningless", and even "useless" and ( $\beta''$ ) "rules of grammar"; and these two expressions were used by him constantly throughout these lectures. And my last puzzle is due to the facts that I think there is reason to suspect that he was not using either expression in any ordinary sense, and that I have not been able to form any clear idea as to how he was using them.

( $\beta'$ ) With regard to the expression "without sense" I think there is no doubt that he was using it in the same way in which he used it in the *Tractatus*, 4.461, when he said that a "tautology" is without sense (*sinnlos*). In that passage he gave as an example of the supposed fact that a "tautology" is without sense the statement "I know nothing about the weather, if I know that either it is raining or it is not"; and in these lectures he used a very similar example to show the same thing. Also in that passage of the *Tractatus* he said that a "tautology" "says nothing", and seemed to mean by this the same as what he meant by saying that it was "without sense"; and this expression he also used in these lectures, and apparently in the same sense. And I think it is clearly true that we could say correctly of a man who only knew that either it was raining or it was not, that he knew nothing about the *present state of the weather*. But could we also say correctly of such a man that he knew *nothing at all*? I do not think we could; and yet, so far as I can see, it is only if we could say this correctly that we should be justified in saying that the sentence "Either it is raining or it is not" "says nothing" or is "without sense". I think, therefore, that Wittgenstein can only have been right in saying that "tautologies" and other sentences, which would commonly be said to express necessary propositions, are "without sense" and "say nothing", if he was using these two expressions in some peculiar way, different from any in which they are ordinarily used. So far as I can see, if we use "make sense" in any way in which it is ordinarily used, "Either it's raining or it's not" *does* make sense, since we should certainly say that the meaning of this sentence is different from that of "Either it's snowing or it's not", thus implying that since they have different meanings, both of them have *some* meaning; and similarly, if "say nothing" is used in any sense in which it is ordinarily used, Wittgenstein's

proposition in *Tractatus* 5.43 that "All the 'Sätze' of Logic say the same, namely, nothing" seems to me to be certainly untrue. And that he was using these expressions in some peculiar way seems to me to be also suggested by the fact that in *Tractatus* 4.461, he seems to be saying that "contradictions" are "without sense" in the same sense in which "tautologies" are, in spite of the fact that in the very same passage he asserts that the latter are "unconditionally true", while the former are "true under no condition". But, if he was using these expressions (and also "meaningless" and "nonsense", which, as I have said, he often used as equivalent to them) in some peculiar sense, what was that sense? Later in (III) he expressly raised the questions "What is meant by the decision that a sentence makes or does not make sense?" and "What is the criterion of making sense?" having said that, in order to answer these questions, he must "plunge into something terrible", and that he must do this in order to "put straight" what he had just been saying, which, he said, he had not "put correctly". In trying to answer these questions or this question (for I think he was using the two expressions to mean the same) he said many things, including the statement that he had himself been "misled" by the expression "sense"; and he went on to say that his present view was that "'sense' was correlative to 'proposition'" (meaning, apparently, here by "proposition" what he had formerly called "proposition in the narrower sense", i.e. "experiential proposition", thus excluding, e.g., mathematical "propositions") and that hence, if "proposition" was not "sharply bounded", "sense" was not "sharply bounded" either. He went on to say about "proposition" the things which I have already quoted (pp. 9-10); and then implied that where we say "This makes no sense" we always mean "This makes nonsense in this particular game"; and in answer to the question "Why do we call it 'nonsense'? what does it mean to call it so?" said that when we call a sentence "nonsense", it is "because of some similarity to sentences which have sense", and that "nonsense always arises from forming symbols analogous to certain uses, where they have no use". He concluded finally that "'makes sense' is vague, and will have different senses in different cases", but that the expression "makes sense" is useful just as "game" is useful, although, like "game", it "alters its meaning as we go from proposition to proposition"; adding that, just as "sense" is vague, so must be "grammar", "grammatical rule" and "syntax".

But all this, it seems to me, gives no explanation of how he was

using the expression "without sense" in the particular case of "tautologies" and other sentences which would commonly be said to express necessary propositions: it only tells us that he might be using it in a different sense in that case from that in which he used it in other cases. The only explanation which, so far as I know, he did give as to how he was using it in the particular case of "tautologies", was where he asked in (III), "What does the statement that a tautology 'says nothing' mean?" and gave as an answer, that to say that " $q \supset q$ " "says nothing" means that  $p. (q \supset q) = p$ ; giving as an example that the logical product "It's raining and I've either got grey hair or I've not" = "It's raining". If he did mean this, and if, as he seemed to be, he was using "says nothing" to mean the same as "is without sense", one important point would follow, namely, that he was not using "without sense" in the same way in the case of "tautologies" as in the case of "contradictions", since he would certainly not have said that  $p. (q \sim q) = p$ . But it gives us no further explanation of how he was using "without sense" in the case of "tautologies". For if he was using that expression in any ordinary way, then I think he was wrong in saying that "It's raining, and I've either got grey hair or I've not" = "It's raining", since, in any ordinary usage, we should say that the "sense" of "either I've got grey hair or I've not" was different from that of, e.g., "either I'm six feet high or I'm not", and should not say, as apparently he would, that both sentences say nothing, and therefore say the same.

In connexion with his use of the phrase "without sense", one other thing which he said or implied more than once should, I think, be mentioned, because it may give a partial explanation of why he thought that both "contradictions" and "tautologies" are without sense. He said in (I) that "the linguistic expression" of "This line can be bisected" is "'This line is bisected' has sense", while at the same time insisting that "the linguistic expression" of "This line is infinitely divisible" is not "'This line is infinitely divided' has sense" (he held that "This line is infinitely divided" is senseless) but is "an infinite possibility in language". He held, therefore, that in many cases the "linguistic expression" of "It is possible that  $p$  should be true" or "should have been true" is "The sentence ' $p$ ' has sense". And I think there is no doubt that he here meant by "possible" what is commonly called, and was called by him on a later occasion, "logically possible". But to say that a sentence " $p$ " is the "linguistic expression" of a *proposition* " $q$ ", would

naturally mean that the sentence " $p$ " and the sentence " $q$ " have the same meaning, although for some reason or other " $p$ " can be called a "linguistic expression"; though the sentence " $q$ " can not. And that he did hold that, if an expression " $p$ " is "the linguistic expression" of a proposition " $q$ ", then the expression " $p$ " and the expression " $q$ " have the same meaning was also suggested by a passage late in (III), where, having explained that by "possible" he here meant "logically possible", he asked the question "Doesn't 'I can't feel his tooth-ache' mean that 'I feel his toothache' has no sense?" obviously implying that the right answer to this question is "Yes, it does". And he also, in several other places, seemed to imply that " $p$  can't be the case", where this means "It is logically impossible that  $p$  should be the case" means the same as "The sentence ' $p$ ' has no sense". I think that his view in the *Tractatus* that "contradictions" are "without sense" (sinnlos) may have been a deduction from this proposition. But why should he have held that "tautologies" also are "without sense"? I think that this view of his may have been, in part, a deduction from the conjunction of the proposition that "It is logically impossible that  $p$ " means the same as "The sentence ' $p$ ' has no sense" with his principle, which I have already had occasion to mention (p. 11), and which he said "gave us some firm ground", that "If a proposition has meaning, its negation also has meaning", where, as I pointed out, he seemed to be using "proposition" to mean the same as "sentence". For it is logically impossible that the negation of a tautology should be true, and hence, if it is true that "It is logically impossible that  $p$ " means the same as "The sentence ' $p$ ' has no sense", then it will follow from the conjunction of this proposition with his principle, that a "tautology" (or should we say "any sentence which expresses a tautology"?) also has none. But why he thought (if he did) that "It is logically impossible that  $p$ " means the same as "The sentence ' $p$ ' has no sense", I cannot explain. And it seems to me that if, as he certainly held, the former of these two propositions entails the latter, then the sentence "It is logically impossible that  $p$ " must also have no sense; for can this sentence have any sense if the sentence " $p$ " has none? But, if "It is logically impossible that  $p$ " has no sense, then, so far as I can see, it is quite impossible that it can mean the same as "The sentence ' $p$ ' has no sense", for this latter expression certainly has sense, if "having sense" is being used in any ordinary way.

( $\beta$ ) With regard to the expressions "rules of grammar" or "grammatical rules" he pointed out near the beginning of (I),

where he first introduced the former expression, that when he said "grammar should not allow me to say 'greenish-red'", he was "making things belong to grammar, which are not commonly supposed to belong to it"; and he immediately went on to say that the arrangement of colours in the colour octahedron "is really a part of grammar, not of psychology"; that "There is such a colour as a greenish blue" is "grammar"; and that Euclidean Geometry is also "a part of grammar". In the interval between (II) and (III) I wrote a short paper for him in which I said that I did not understand how he was using the expression "rule of grammar" and gave reasons for thinking that he was not using it in its ordinary sense; but he, though he expressed approval of my paper, insisted at that time that he was using the expression in its ordinary sense. Later, however, in (III), he said that "any explanation of the use of language" was "grammar", but that if I explained the meaning of "flows" by pointing at a river "we shouldn't naturally call this a rule of grammar". This seems to suggest that by that time he was doubtful whether he was using "rule of grammar" in quite its ordinary sense; and the same seems to be suggested by his saying, earlier in (III), that we should be using his "jargon" if we said that whether a sentence made sense or not depended on "whether or not it was constructed according to the rules of grammar".

I still think that he was not using the expression "rules of grammar" in any ordinary sense, and I am still unable to form any clear idea as to how he was using it. But, apart from his main contention (whatever that may have been) as to the connexion between "rules of grammar" (in his sense) and necessary propositions, there were two things upon which he seemed mainly anxious to insist about "rules of grammar", namely ( $\gamma'$ ), that they are all "arbitrary" and ( $\gamma''$ ) that they "treat only of the symbolism"; and something ought certainly to be said about his treatment of these two points.

As for ( $\gamma'$ ) he often asserted without qualification that all "rules of grammar" are arbitrary. But in (II) he expressly mentioned two senses of "arbitrary" in which he held that some grammatical rules are *not* arbitrary, and in one place in (III) he said that the sense in which all were arbitrary was a "peculiar" one. The two senses, of which he said in (II) that some grammatical rules were *not* arbitrary in those senses, were (1) a sense in which he said that rules about the use of single words were always "in part" *not* arbitrary—a proposition which he thought followed from his proposition, which I have mentioned before

(p. 7), that all single words are significant only if "we commit ourselves" by using them, and (2) a sense in which to say that a rule is an established rule in the language we are using is to say that it is not arbitrary: he gave, as an example, that if we followed a rule according to which "hate" was an intransitive verb, this rule would be arbitrary, whereas "if we use it in the sense in which we do use it", then the rule we are following is not arbitrary. But what, then, was the sense in which he held that all grammatical rules *are* arbitrary? This was a question to which he returned again and again in (II), trying to explain what the sense was, and to give reasons for thinking that in that sense they really are arbitrary. He first tried to express his view by saying that it is impossible to "justify" any grammatical rule—a way of expressing it to which he also recurred later; but he also expressed it by saying that we can't "give reasons" for grammatical rules, soon making clear that what he meant by this was that we can't give reasons for *following* any particular rule rather than a different one. And in trying to explain why we can't give reasons for following any particular rule, he laid very great stress on an argument, which he put differently in different places, and which I must confess I do not clearly understand. Two of the premisses of this argument are, I think, clear enough. One was (1) that any reason "would have to be a description of reality": this he asserted in precisely those words. And the second was (2) that "any description of reality must be capable of truth and falsehood" (these again were his own words), and it turned out, I think, that part of what he meant by this was that any false description must be significant. But to complete the argument he had to say something like (what again he actually said in one place) "and, if it were false, it would have to be said in a language not using this grammar"; and this is what I do not clearly understand. He gave as an illustration of his meaning that it cannot be because of a "quality in reality" that "I use sweet" in such a way that "sweeter" has meaning, but "identical" in such a way that "more identical" has none; giving as a reason "If it were because of a 'quality' in reality, it must be possible to say that reality hasn't got this quality, which grammar forbids". And he had said previously "I can't say what reality would have to be like, in order that what makes nonsense should make sense, because in order to do so I should have to use this new grammar". But, though I cannot put clearly the whole of his argument, I think one important point results from what I have quoted—a point which he himself never expressly pointed out. It results,



namely, that he was using the phrases "description of reality" and "quality in reality" in a restricted sense—a sense, such that no statement to the effect that a certain expression is actually used in a certain way is a "description of reality" or describes "a quality in reality". He was evidently so using these terms that statements about the actual use of an expression, although such statements are obviously experiential propositions, are not to be called "descriptions of reality". He was confining the term "descriptions of reality" to expressions in which no term is used as a name for itself. For if he were not, it is obviously perfectly easy to say what reality would have to be like in order that "more identical", which is nonsense, should make sense: we can say that if "more identical" were used to mean what we now mean by "sweeter", then it would make sense; and the proposition that "more identical" is used in that way, even if it is a false one (and I do not know for certain that the very words "more identical" are not used in that way in *e.g.* some African language) it is certainly not one which English grammar "forbids" us to make—it is certainly untrue that the sentence which expresses it has no significance in English.

It seems, therefore, that though in (II) he had said that what he meant by saying that all "grammatical rules" are "arbitrary" was that we cannot "give reasons" for following any particular rule rather than a different one, what he meant was only that we cannot give reasons for so doing which are both (a) "descriptions of reality" and (b) "descriptions of reality" of a particular sort, *viz.* descriptions of reality which do not mention, or say anything about, any particular word or other expression, though of course they must use words or other expressions. And that this was his meaning is made, I think, plainer from a passage late in (III) in which he compared rules of deduction with "the fixing of a unit of length" (or, as he said later, a "standard" of length). He there said "The reasons (if any) for fixing a unit of length do not make it 'not arbitrary', in the sense in which a statement that so and so is the length of this object is not arbitrary", adding "Rules of deduction are analogous to the fixing of a unit of length", and (taking " $3 + 3 = 6$ " as an instance of a rule of deduction) " $3 + 3 = 6$ " is a rule as to the way we are going to talk . . . it is a preparation for a description, just as fixing a unit of length is a preparation for measuring". He seemed, therefore, here to be admitting that reasons of a sort can sometimes be given for following a particular "grammatical rule", only not reasons of the special sort which a well-conducted operation of measurement may give (once the meaning of "foot"

has been fixed), for, *e.g.*, the statement that a particular rod is less than four feet long. He did in fact mention in this connexion that some "grammatical rules" follow from others; in which case, of course, that they do so follow may be given as a reason for speaking in accordance with them. In this case, however, he would no doubt have said that the reason given is not a "description of reality". But it is obvious that reasons which are, in any ordinary sense, "descriptions of reality" can also be given for following a particular rule; *e.g.* a particular person may give, as a reason for calling a particular length a "foot", the "description of reality" which consists in saying that that is how the word "foot", when used for a unit of length, is generally used in English. And, in this case, of course, it may also be said that the reason why the word "foot" was originally used, in English, as a name for the particular length which we do in fact so call, was that the length in question is not far from the length of those parts of a grown man's body which, in English, are called his "feet". In these cases, however, I think he might have urged with truth both (a) that the reason given, though a "description of reality", is a description which "mentions" or says something *about* the word "foot" and does not merely *use* that word, and also (b) that it is not a reason for following the rule of calling that particular length a "foot" in the same sense of the word "reason" as that in which a well-conducted measurement may give a "reason" for the statement that a particular rod is less than four feet long. It is surely obvious that a "reason" for *acting* in a particular way, *e.g.* in this case, for using the word "foot" for a particular length, cannot be a reason for so doing in the same sense of the word "reason" as that in which a reason for thinking that so and so is the case may be a reason for so thinking. I think, therefore, if all these explanations are given, it becomes pretty clear in what sense Wittgenstein was using the word "arbitrary" when he said that all grammatical rules were arbitrary.

But there remains one thing which he said in this connexion which has puzzled me extremely. He actually introduced his comparison between rules of deduction and the fixing of a unit of length by saying: "The statement that rules of deduction are neither true nor false is apt to give an uncomfortable feeling." It appeared, therefore, as if he thought that this statement that they are neither true nor false followed from the statement that they are arbitrary, and that the comparison of them with the fixing of a unit of length would tend to remove this uncomfortable feeling, *i.e.* to make you see that they really are neither true nor false.

Now, in connexion with his comparison between rules of deduction and the fixing of a unit of length, he gave (among other examples) as an example of a rule of deduction " $3 + 3 = 6$ ", and said a good deal about this example. And it certainly does give me a very uncomfortable feeling to be told that " $3 + 3 = 6$ " is neither true nor false. But I think this uncomfortable feeling only arises because one thinks, if one is told this, that the expression " $3 + 3 = 6$ " is being used in the way in which it most commonly is used, *i.e.* in the way in which it would commonly be said to be expressing a necessary proposition. And I think this uncomfortable feeling would completely vanish if it were clearly explained that the person who says this, is *not* using the expression " $3 + 3 = 6$ " in this way, but in the very different way which I tried to distinguish above (p. 291), *i.e.* the way in which it is merely used to specify a possible way of speaking and writing, which might or might not be actually adopted, although in this case the rule of speaking and writing in the way specified is, as a matter of fact, a well-established rule. I said that Wittgenstein never, so far as I knew, in these lectures expressly distinguished these two different ways of using the same expression (*e.g.* the expression " $3 + 3 = 6$ "), but that I thought he did hold that, *e.g.* the expression " $3 + 3 = 6$ " could be properly used in the second way as well as in the first, and that his thinking this might be partly responsible for his declaration that rules of deduction are neither true nor false (p. 291). For it seemed to me quite obvious that, if the expression " $3 + 3 = 6$ " is used in this second way, then it cannot possibly be either true or false. But I cannot help thinking that in this passage in (III) in which he compared rules of deduction with the fixing of a unit of length, he actually meant to say that even when used in the first way, *i.e.* in the way in which it would commonly be said to express a necessary proposition, it still expresses neither a true nor a false proposition.

In what he actually said about " $3 + 3 = 6$ " in this passage, I think it is necessary to distinguish three different propositions which he made, of which the first two seem to me certainly true, but the third not to follow from the first two, and to be extremely doubtful. (1) He began by asking the question, "Is 'I've put 6 apples on the mantel-piece' the same as 'I've put 3 there, and also another 3 there'?" and then, after pointing out that counting up to 3 in the case of each of two different groups, and arriving at the number "6" by counting *all* the apples, are "three different experiences", he said "You can imagine putting two groups of 3 there, and then finding only 5". And the two

propositions which seem to me certainly true are (a) that you can imagine this which he said you can imagine, and (b) (which he also said) that " $3 + 3 = 6$ " does not "prophesy" that, when you have had the two experiences of counting up to 3 in the case of each of two groups of apples which you certainly have put on the mantel-piece, you will also have the third experience of finding that there are 6 there, when you come to count *all* the apples that are there; or, in other words, he was saying that the proposition " $3 + 3 = 6$ " is quite consistent with finding, by your third experience of counting, that there are only 5 there. This second proposition seems to me also true, because it seems to me clear that it is a mere matter of experience, that when you have put two groups of 3 apples on a mantel-piece, you will, under the circumstances Wittgenstein was considering (e.g. that no apple has been taken away) find that there are 6 there; or, in other words, it is a mere matter of experience that apples don't simply vanish with no apparent cause; and it surely should be obvious that " $3 + 3 = 6$ " certainly entails no more than that, *if* at any time there were in any place two different groups, each numbering 3 apples, then *at that time* there were 6 apples in the place in question: it entails nothing about any future time. But (2) Wittgenstein went on to add that if, on having put two groups of 3 on the mantel-piece, and finding that there were only 5 there, you were to say (as you certainly might under the circumstances he was considering) "one must have vanished", this latter statement "*only means* 'If you keep to the arithmetical rule " $3 + 3 = 6$ "' you *have to say* 'One must have vanished'". And it is this assertion of his that, under the supposed circumstances, "One must have vanished" *only means* that, if you keep to a certain rule, you must *say so*, which seems to me questionable and not to follow from the two true propositions I have given under (1). (He had already said something similar in (I) in connexion with his very paradoxical proposition that Euclidean Geometry is "a part of grammar"; for he there said that what Euclid's proposition "The three angles of a triangle are equal to two right angles" asserts is "If by measurement you get any result for the sum of the three angles other than  $180^\circ$ , you *are going to say* that you've made a mistake".)

But I have been a good deal puzzled as to what he meant and implied by this assertion that, under these circumstances the words "One must have vanished" *only mean* "If you keep to the arithmetical rule ' $3 + 3 = 6$ ', you have to say so". And, of course, my view that it is very doubtful whether what he

meant and implied is true depends on my view as to what he did mean and imply.

Of course, the circumstances under which he said that "One must have vanished" *only means* this, are extremely unusual: possibly they never have happened and never will happen: but, as I have said, I fully agree with him that they *might* happen—that I can *imagine* their happening; and the question whether, if they did, then the words "One must have vanished" would *only mean* what he says, seems to me to raise an extremely important question, which does not only concern what would happen under these extremely unlikely circumstances, but concerns what is the case under circumstances which constantly do occur.

I will first try to state as accurately as I can what I take to be the circumstances he was supposing, and I will put them in the form of what he was supposing would be true of me, if I had been in those circumstances. He was supposing, I take it, (1) that I should know, because I had counted correctly, that I did put on the mantel-piece two groups of apples, each of which contained 3 apples and no more, (2) that I should know, by counting *at a subsequent time*, that there were *at that time* only 5 apples on the mantel-piece, (3) that I should also know, because I was watching all the time, that nothing had happened which would account in any normal way for the fact that, though I put  $3 + 3$  there, there are only 5 there now, *e.g.* I should know, in this way, that nobody had taken one away, that none had fallen off the mantel-piece, and that none had visibly flown away, and finally, (4) (and this, if I am not mistaken, was very essential to his point) that I should *not* know, by any operation of counting performed either by myself or by any other person who had told me his result, that I did put 6 apples on the mantel-piece, so that, if I asserted that I did put 6, this could only be a deduction from the proposition that I did put  $3 + 3$  there, which (1) asserts that I *have* found to be true by counting.

Under the circumstances stated in (1) and (3) I certainly should be very much surprised to find that what is stated in (2) was true, and I might quite naturally assert that one must have vanished, though I think I might equally naturally express my surprise by the use of words which contain no "must", *e.g.* by saying "Why! one has vanished!" And it is under these circumstances, if I am not mistaken, that Wittgenstein was asserting that if I did use the words "One must have vanished" to make an assertion, these words would "*only mean*" "If I keep to the arithmetical rule ' $3 + 3 = 6$ ' I have to say that one must have vanished".

And, first of all, I have felt some doubts on two separate points as to what he meant by the words "you have to say so". The first is this. I at first thought that he might be using the words "say so", rather incorrectly, to mean "say *the words* 'One must have vanished'" (or, of course, any equivalent words, e.g. in another language). But I now think there is no reason to suppose that he was not using the word "say" quite correctly (i.e. as we usually do), to mean the same as "assert", and there is some positive reason to suppose that he was doing so. One positive reason is that, among the circumstances which he was supposing was that which I have called (1), viz. that I should *know* that I did put  $3 + 3$  apples on the mantel-piece; and I think he was certainly supposing that if I knew this, I should not merely say the words "I put  $3 + 3$  there", but should *assert* that I did—a proposition which does not seem to me certainly true, though, if I knew it, I should certainly be *willing* to assert it, unless I wanted to tell a lie. The second point is this: What did he mean by "have to" in "I should *have to* say so"? These words might naturally mean that I should be failing to keep to the rule " $3 + 3 = 6$ " if I merely failed to assert that one must have vanished—if, for instance, I merely made no assertion. But I feel sure he did not mean to assert that I should be failing to keep to the rule (which I will in future call, for short, "violating" the rule) if I merely omitted to say that one must have vanished. I think he certainly meant that I should be violating the rule only if I made some assertion the making of which was inconsistent with asserting that one must have vanished, e.g. if I asserted that none had vanished.

If I am right on these two points, his view as to what the words "One must have vanished" would "only mean" under the supposed circumstances, could be expressed more clearly as the view that these words would only mean "If I assert that I put  $3 + 3$  on the mantel-piece, I shall be keeping to the rule " $3 + 3 = 6$ " if I also assert that one must have vanished, and shall be violating that rule, if I make any assertion the making of which is inconsistent with asserting that one must have vanished". And I will, in future, assume that this was his view.

But now the question arises: Why should he have held that, under the supposed circumstances, the words "One must have vanished" would "only mean" a proposition which mentioned the arithmetical proposition " $3 + 3 = 6$ "? How does "6" come in? I think the answer to this question is that he was assuming that among the propositions from which, under the

supposed circumstances, the proposition that one had vanished would be a deduction (the "must have", of course, indicates, as "must" often does, that it would be a deduction from *some* propositions) would be not only the propositions given as known by me in (1), (2) and (3) of my description of the circumstances, but also the proposition "I put 6 apples on the mantel-piece", which, according to (4) in my description is *not* known by me as a result of any operation of counting, but only, if at all, as a deduction from "I put  $3 + 3$ ". And I think his reason for asserting that, under the supposed circumstances "One must have vanished" would "only mean" what he said it would only mean, was that he was supposing that this sentence "I put 6 on the mantel-piece" would *not*, under the circumstances described in (1) and (4), mean what it would mean if I had discovered, by counting *all* the apples I was putting on the mantel-piece, that I was putting 6, but, since I had not done this, would "only mean" "I shall be keeping to the rule ' $3 + 3 = 6$ ', if I assert that I put 6, and shall be violating that rule if I make any assertion inconsistent with asserting that I put 6"—a proposition which to avoid clumsy repetitions, I will in future call "B". I think, therefore, he was implying that under the circumstances (1) and (4), the words "I put 6 apples on the mantel-piece" would "only mean" B. And I think the important question raised by his assertion as to what "One must have vanished" would "only mean" under circumstances (1), (2), (3) and (4), is this question as to whether, under circumstances (1) and (4), which might quite often occur, "I put 6" would "only mean" B—"only mean" being used, of course, in the same sense (whatever that may have been) in which he used it with regard to "One must have vanished".

But then the question arises : In what sense was he using the expression "only mean" ? If any one tells us that, under the circumstances (1) and (4), the sentence "I put 6 on the mantel-piece", if used to make an assertion, would "only mean" B, I think the most natural interpretation of these words would be that anyone who, under circumstances (1) and (4), used this sentence to make an assertion would be using it to assert B. But I think it is quite incredible that any one would ever actually use the expression "I put 6" to assert B ; and equally incredible that any one would ever use the expression "One must have vanished" to assert what Wittgenstein said would be their "only meaning" under circumstances (1), (2), (3) and (4). In both cases the assertion which is said to be the "only meaning" of a given expression is an assertion *about* the ordinary meaning



of the expression in question, to the effect that you will be speaking in accordance with a certain rule if you use the expression in question to assert what it would be usually used to mean, and will be violating that rule if you make any assertion inconsistent with that ordinary meaning. And I think it is quite incredible that anybody would ever use a given expression to make such an assertion *about* the ordinary meaning of the expression in question; and, in both our cases, quite clear that anybody who did use the expression in question to make an assertion, would be using it to *make* the assertion which it would ordinarily mean, and not to make the assertion *about* its ordinary meaning which Wittgenstein said or implied would be its "only meaning" under the circumstances described. And I do not think that he ever meant to make either of these incredible statements: he was not intending to say that the sentence "One must have vanished" ever would be used, or could be properly used, to make the assertion which he says would be its "only meaning" under the supposed circumstances. But, if so, how was he using the expression "only mean"? I think he was using it, not in its most natural sense, but loosely, in a more or less natural sense, to say that the assertion which he said would be its "only meaning" under the circumstances described, would be *the* true proposition which resembled most closely a proposition which he held to be false, but which he knew was commonly held to be true. In the case of "I put 6", if he implied, as I think he did, that, under circumstances (1) and (4), "I put 6" would "only mean" B, the proposition which he held to be false was the proposition that if I put  $3 + 3$ , it is *necessarily* also true that I put 6, and the proposition which he held to be *the* true proposition which most closely resembled this false proposition, and which might therefore have misled those who think to be true this proposition which he held to be false, was that, if "I put  $3 + 3$ " was true, then B would be true. I think, in fact, he was holding that the proposition that in putting  $3 + 3$  on the mantel-piece, I was *necessarily* putting 6 there, was false; that I can imagine that in putting  $3 + 3$  there, I was *e.g.* only putting 5, and that, if there ever were  $3 + 3$  on the mantel-piece, nevertheless, if anybody had counted correctly how many there were altogether *at that very time*, he would possibly have found that there were only 5.

But whether or not he held, as I think he did, that in putting  $3 + 3$  apples on the mantel-piece, I was not necessarily putting 6, I think it is quite certain that he held another proposition, about the relation of which to this one I am not clear. He held,

namely, that the expression " $3 + 3 = 6$ " is *never* used in Arithmetic, not therefore even when it would commonly be held to express a necessary proposition, to express a proposition from which it follows that if I put  $3 + 3$ , I necessarily put 6. And this view seems to me to follow from his two views (1) that (as is suggested by his phrase "the arithmetical rule ' $3 + 3 = 6$ '") the expression " $3 + 3 = 6$ ", as used in Arithmetic, *always* only expresses a "rule of grammar", and (2) that rules of grammar "treat only of the symbolism". I shall shortly have to point out that there seems to me to be a serious difficulty in understanding exactly what he meant by saying that, e.g. " $3 + 3 = 6$ " "treats only of the symbolism"; but I think there is no doubt he meant at least this: that you will be speaking in accordance with that rule if, when you *assert* that you put  $3 + 3$ , you also *assert* that you put 6, and violating it if, having *asserted* the former, you make any assertion the making of which is incompatible with asserting the latter, but that it by no means follows that, if you keep to the rule, what you assert will be *true*, nor yet that, if you violate it, what you assert will be *false*: in either case, he held, what you assert *may* be true, but also *may* be false. And I think his reason for this view of his can be made plainer by noticing that since (as he implies by his phrase "the arithmetical rule ' $3 + 3 = 6$ '") " $3 + 3 = 6$ " is a well-established rule (if a rule of grammar at all), it will follow that, if you keep to that rule, you will be using language "correctly" (or, with his use of "grammar", speaking "grammatically"), and that, if you violate it will be speaking "incorrectly" (or, with his use of "grammar", guilty of bad grammar); and that from the fact that you are using language correctly, in the sense of "in accordance with an established rule", it by no means follows that what you assert, by this correct use of language, is "correct" in the very different sense in which "That is correct" = "That is true", nor from the fact that you are using language incorrectly that what you assert by this incorrect use of language is "incorrect" in the very different sense in which "That is incorrect" = "That is false". It is obvious that you may be using language just as correctly when you use it to assert something false as when you use it to assert something true, and that when you are using it incorrectly, you may just as easily be asserting something true by this incorrect use as something false. It by no means follows, for instance, from the fact that you are using the word "foot" "correctly", i.e. for the length for which it is usually used in English, that when you make such an assertion as "This rod is

less than four feet long", your assertion is true; and, if you were to use it "incorrectly" for the length which is properly called in English an "inch" or for that which is properly called a "yard", it would by no means follow that any assertion you made by this incorrect use of the word "foot" was false. I think Wittgenstein thought that similarly you will be using the phrase "I put 6" correctly, if, when you assert that you put  $3 + 3$ , you also assert that you put 6, and incorrectly if, when you assert that you put  $3 + 3$ , you deny that you put 6, or even assert that it is possible that you did not put 6; and that this is *the* true proposition which has led people to assume, what he thought false, that the expression " $3 + 3 = 6$ " is used in Arithmetic to express a proposition from which it follows that if I put  $3 + 3$ , I necessarily put 6.

And I think this view of his also gives the chief explanation of what he meant by the puzzling assertion that  $3 + 3 = 6$  (and *all* rules of deduction, similarly) is neither true nor false. I think what he chiefly meant by saying this was not, as I suggested above (p. 291) that  $3 + 3 = 6$  was a "rule" in the sense in which rules can obviously be neither true nor false, but that he was using "true" in a restricted sense, in which he would have said that  $3 + 3 = 6$  was only "true" if it followed from it (as he denied to be the case) that if I put  $3 + 3$  on the mantel-piece, I necessarily put 6; in a sense, therefore, in which, even if, as I suggested (p. 291), he sometimes used "rule" in a sense in which the proposition "You can't *say* that you put  $3 + 3$ , and *deny* that you put 6"—a proposition which he held to be true in any ordinary sense, he would nevertheless have said that this proposition was not "true", because it was a proposition about how words are actually used. I think he was using "true" and "false" in a restricted sense, just as he was using "description of reality" in a restricted sense (above, p. 300), *i.e.* in a sense in which no propositions about how words are used can be said to be "true" or "false".

And the reason why I think it very doubtful whether he was right in holding (if he did hold) that it is not true that in putting  $3 + 3$  on the mantel-piece, I necessarily also put 6, is that I do not think I can imagine that in putting  $3 + 3$ , I was not putting 6. I have already said (p. 303) that I agree with him that I can imagine that, having put  $3 + 3$  there, I should find at a *subsequent time* that there were only 5 there, even under the circumstances described in (3) of my description of the circumstances he was supposing: I can imagine, I think, that one has really vanished. But it seems to be quite a different question whether I can

imagine that, in putting  $3 + 3$ , I was not putting 6, or that, if at any time there were  $3 + 3$  on the mantel-piece, there were at that time not 6 there. I admit, however, that the propositions that I was putting 6, or that there were 6 on the mantel-piece, do seem to me to entail that, if anybody had counted correctly, he would have found that there were 6; I am, therefore, implying that I cannot imagine these hypothetical propositions not to be true: but I do not think I can imagine this. And I also can see no reason to think that the expression " $3 + 3 = 6$ " is never used in Arithmetic to express a proposition from which it follows that if I put  $3 + 3$ , I put 6. I am not convinced that this expression, in Arithmetic, *always* only expresses a "grammatical rule", i.e. a rule as to what language it will be correct to use, even if it sometimes does. Wittgenstein has not succeeded in removing the "uncomfortable feeling" which it gives me to be told that " $3 + 3 = 6$ " and " $(p \supset q \cdot p)$  entails  $q$ " are neither true nor false.

( $\gamma'$ ) As for the proposition that rules of grammar "treat only of the symbolism", he never, at least while I was present, expressly pointed out that such an expression as " $2 = 1 + 1$ " can be used to express at least three very different propositions. It can be used (1) in such a way that anybody could understand what proposition or rule it was being used to express, provided only he understood how the sign " $=$ " was being used, and did not understand either the expression " $2$ " or the expression " $1 + 1$ " except as names for themselves (what has been called "autonomously"). But it can be used (2) in such a way that nobody could understand what proposition or rule it was being used to express, unless he understood non-autonomously both the sign " $=$ " and also the expression " $1 + 1$ ", but need not understand the expression " $2$ " other than autonomously. Or (3) it can be used in such a way that nobody could understand what proposition or rule it was being used to express, unless he understood non-autonomously *both* the expression " $2$ " and the expression " $1 + 1$ ", as well as the expression " $=$ ". But, though he did not expressly point out that e.g. " $2 = 1 + 1$ " could be used in each of these three very different ways, he said things which seem to me to imply the view that in Arithmetic it was *only* used in the first way. He said, for instance, in (II) "To explain the meaning of a sign means only to substitute one sign for another", and again, later on, "An explanation of a proposition is always of the same kind as a definition, i.e. replacing one symbol by another". In making these statements, he seems to me to have been confusing the true proposition that

you can only explain the meaning of one sign by *using* other signs, with the proposition, which seems to me obviously false, that, when you explain the meaning of one sign by *using* another, all you are asserting is that the two signs have the same meaning or can be substituted for one another: he seems in fact to have been asserting that propositions, which are in fact of form (2), are only of form (1). And this mistake seems to be responsible for the astounding statement which he actually made in (III) that Russell had been mistaken in thinking that " $=$  Def." had a different meaning from " $=$ ". It seems to me obvious that a statement can only be properly called a "definition" or "explanation" of the meaning of a sign, if, in order to understand what statement you are making by the words you use, it is necessary that the hearer or reader should understand the *definiens*, and not merely take it as a name for itself. When, for instance, *Principia Mathematica* defines the meaning of the symbol " $\supset$ " by saying that " $p \supset q$ " is to mean " $\sim p \vee q$ ", it is surely obvious that nobody can understand what statement is being made as to how " $\supset$ " will be used, unless he understands the expression " $\sim p \vee q$ ", and does not take it merely as a name for itself; and that therefore the statement which is being made is not merely a statement of form (1), to the effect that the two different expressions " $p \supset q$ " and " $\sim p \vee q$ " have the same meaning or can be substituted for one another, but a statement of form (2), i.e. that the *definiens* " $\sim p \vee q$ " is *not* being used autonomously, though the *definiendum* " $\supset$ " is being used autonomously.

But the most serious difficulty in understanding what he meant by saying that, e.g. " $3 + 3 = 6$ " "treats only of the symbolism" seems to me to arise from a question with which he only dealt briefly at the end of (I), and with which he there dealt only in a way which I certainly do not at all completely understand; namely, the question: Of *what* symbols did he suppose that " $3 + 3 = 6$ " was treating? He did indeed actually assert in (III) that the proposition "red is a primary colour" was a proposition about the word "red"; and, if he had seriously held this, he might have held similarly that the proposition or rule " $3 + 3 = 6$ " was merely a proposition or rule about the particular expressions " $3 + 3$ " and " $6$ ". But he cannot have held seriously either of these two views, because the *same* proposition which is expressed by the words "red is a primary colour" can be expressed in French or German by words which say nothing about the English word "red"; and similarly the *same* proposition or rule which is expressed by

" $3 + 3 = 6$ " was undoubtedly expressed in Attic Greek and in Latin by words which say nothing about the Arabic numerals "3" and "6". And this was a fact which he seemed to be admitting in the passage at the end of (I) to which I refer. In this passage, which he introduced by saying that he would answer objections to the view (which he held) that the arithmetical calculus "is a game", he began by saying, very emphatically, that it is *not* a game "with ink and paper"; by which he perhaps meant (but I do not know) that it is not a game with the Arabic numerals. He went on to say that Frege had concluded from the fact that Mathematics is not a game "with ink and paper" that it dealt not with the symbols but with "what is symbolised"—a view with which he apparently disagreed. And he went on to express his own alternative view by saying "What is essential to the rules is the logical multiplicity which all the different possible symbols have in common"; and here, by speaking of "all the different possible symbols", I take it he was admitting, what is obvious, that the *same* rules which are expressed by the use of the Arabic numerals may be expressed by ever so many different symbols. But if the rules "treat only of the symbolism" how can two rules which treat of *different* symbols, *e.g.* of "3" and "III", possibly be the *same* rule? I suppose he must have thought that we use the word "same" in such a sense that two rules, which are obviously *not* the same, in that they treat of different symbols, are yet said to be the same, provided only that the rules for their use have the same "logical multiplicity" (whatever that may mean). But he never, I think, at least while I was present, returned to this point, or tried to explain and defend his view.

He did, however, in this passage, compare the rules of Arithmetic to the rules of chess, and used of chess the phrase, "What is characteristic of chess is the logical multiplicity of its rules" just as he used of Mathematics the phrase "What is essential to its rules is the logical multiplicity which all the different possible symbols have in common". I doubt, however, whether he was right in what he meant by saying "*What* is characteristic of chess is the logical multiplicity of its rules", which, of course, implies that this is sufficient to characterise chess. He was undoubtedly right in saying that the material and the shape of which the different pieces are commonly made is irrelevant to chess: chess could certainly be played with pieces of any material and any shape, *e.g.* with pieces of paper which were all of the same shape. But if by "the rules of chess" he meant, as I think he probably did, the rules which govern the moves which

may be made by pieces of different sorts, *e.g.* by pawns and bishops, and was suggesting that the "logical multiplicity" of the rules which govern the possible moves of a pawn and a bishop is sufficient to distinguish a pawn from a bishop, I think he was wrong. The rule that a pawn can only make certain moves certainly, I think, does not mean that any piece the rules for the moves of which have a certain "logical multiplicity" (whatever that may mean) may only make the moves in question, even if he was right in holding that the rules for the moves of pawns have a different "logical multiplicity" from those for the moves of bishops; and similarly in the case of all the other different kinds of pieces. Though a pawn is certainly not necessarily distinguished from a bishop or a knight by its shape, as it usually is, it seems to me that it is necessarily distinguished by the positions which it may occupy at the beginning of the game, so that a rule which states that pawns can only make such and such moves, states that pieces which occupy certain positions at the beginning of the game can only make such and such moves; and similarly with all the other different kinds of piece: they are all necessarily distinguished from one another by the positions which they occupy at the beginning of the game, where "necessarily" means that it would not be chess that you were playing, if the pieces to which different kinds of move are allowed, did not occupy certain positions relatively to one another at the beginning of the game. Of course, if you did play chess with pieces of paper which were all of the same shape, it would be necessary that the pieces should have some mark to show what positions they had occupied at the beginning of the game, as might be done, for instance, by writing "pawn" on those pieces which had occupied certain positions, and, *e.g.* "bishop" on those which had occupied others; and it would also be necessary to distinguish by some mark (what is usually done by a difference of colour) those pieces which belonged to one of the two players from those which belonged to the other, as could, *e.g.* be easily done by writing a "0" on all the pieces which belonged to one player, and a "+" on all which belonged to the other. I think, therefore, he was probably wrong in holding, as he apparently did, that the rules of chess are completely analogous, in respect of their relation to "logical multiplicity", to what he held to be true of the rules of Arithmetic.

There remains one other matter which should be mentioned in treating of his views about necessary propositions. He made a good deal of use, especially in (II) in discussing rules of deduction,



of the expression "internal relation", even asserting in one place "What justifies inference is an internal relation". He began the discussion in which he made this assertion by saying that "following" is called a "relation" as if it were like "fatherhood"; but said that where, for example, it is said that a proposition of the form " $p \vee q$ " "follows" from the corresponding proposition of the form " $p \cdot q$ ", the so-called "relation" is "entirely determined by the two propositions in question", and that, this being so, the so-called "relation" is "entirely different from other relations". But it soon became plain that, when he said this about "following", it was only one of the proper uses of the word "follow" in English, as between two propositions, of which he was speaking, namely, that use which is sometimes called "follows logically": he did, in fact, constantly use the word "inference" as if it meant the same as "deductive inference". How he made plain that what he was talking of as "following" was only "following logically", was that he immediately went on to say that the kind of "following" of which he was speaking, and which he exemplified by the sense in which any proposition of the form " $p \vee q$ " "follows" from the corresponding proposition of the form " $p \cdot q$ ", was "quite different" from what is meant when, *e.g.* we say that a wire of a certain material and diameter *can't* support a piece of iron of a certain weight—a proposition which he actually expressed in the next lecture (quite correctly according to English usage) as the proposition that "it *follows* from the weight of the piece of iron and the material and diameter of the wire, that the wire will break if you try to support that piece of iron by it". He went on to express the difference between these two uses of "follow", by saying that, in the case of the wire and the piece of iron, both (a) "it remains *thinkable* that the wire will not break", and (b) that "from the weight of the piece of iron and the material and diameter of the wire *alone*, I can't know that the wire will break", whereas in the case of a proposition of the form " $p \vee q$ " and the corresponding proposition of the form " $p \cdot q$ " "following" is an "internal relation", which, he said, means "roughly speaking" "that it is *unthinkable* that the relation should not hold between the terms". And he immediately went on to say that the *general* proposition " $p \vee q$  follows from  $p \cdot q$ " "is not wanted"; that "if you can't see", by looking at two propositions of these forms that the one follows from the other, "the general proposition won't help you"; that, if I say of a proposition of the form " $p \vee q$ " that it follows from the corresponding proposition of the form " $p \cdot q$ " "everything here is

useless, except the two propositions themselves"; and that if another proposition were needed to justify our statement that the first follows from the second, "we should need an infinite series". He finally concluded "A rule of inference" (meaning "deductive inference") "never justifies an inference".

In the next lecture, which he began, as he often did, by repeating (sometimes in a slightly different form and, if necessary, with added explanations and corrections) the main points which he had intended to make at the end of the preceding one, he said that to say of one proposition " $q$ " that it "follows" from another " $p$ " "*seems* to say that there is a relation between them which justifies passing from one to the other", but that "what makes one suspicious about this is that we perceive the relation by merely looking at the propositions concerned—that it is 'internal' and not like the proposition that 'This wire will break' follows from the weight of the iron and the material and diameter of the wire"; and here he immediately went on to add that the expression "internal relation" is misleading, and that he used it "only because others had used it"; and he proceeded to give a slightly different formulation of the way in which the expression had been used, *viz.* "A relation which holds if the terms are what they are, and which cannot therefore be imagined not to hold". He also, shortly afterwards, gave some further explanation of what he had meant by saying that if a rule were needed to justify the statement that one proposition follows [logically] from another, we should need an infinite series. He said that if a rule  $r$ , were needed to justify an inference from  $p$  to  $q$ ,  $q$  would follow from the conjunction of  $p$  and  $r$ , so that we should need a fresh rule to justify the inference from this conjunction to  $q$ , and so on *ad infinitum*. Hence, he said, "an inference can only be justified by what we see", and added that "this holds throughout Mathematics". He then gave his truth-table notation for " $p \vee q$ " and " $p \cdot q$ ", and said that the "criterion" for the statement that the former follows from the latter was that "to every T in the latter there corresponds a T in the former". He said that, in saying this, he had stated "a rule of inference", but that this rule was only a "rule of grammar" and "treated only of the symbolism". A little later he said that the relation of "following" can be "represented" by "tautologies" (in his special sense), but that the tautology " $(p \cdot q) \supset (p \vee q)$ " does not say that  $p \vee q$  follows from  $p \cdot q$ , because it says nothing, but that the fact that it is a tautology *shows* that  $p \vee q$  follows from  $p \cdot q$ . And a little later still he said that the relation of following "can be seen by

looking at the *signs*", and seemed to identify this with saying that it is "internal"; and the fact that he here said that it can be seen by looking at the *signs*, whereas he had previously said that it can be seen by looking at the *propositions*, seems to me to shew that, as I said (p. 11) seemed to be often the case, he was identifying "sentences" with "propositions". Finally he introduced a new phrase, in explanation of his view that the expression "internal relation" is misleading, saying that internal and external relations are "categorically" different; and he used the expression "belong to different categories" later on in (III), where he said that "follows" and "implies" (a word which he here used, as Russell had done, as if it meant the same as the *Principia* symbol " $\supset$ ") "belong to different categories"; adding the important remark that whether one proposition "follows" from another "cannot depend at all upon their truth or falsehood", and saying that it only depends on "an internal or grammatical relation".<sup>1</sup>

(To be continued)

<sup>1</sup> My attention has been called to the fact that in my last article (*MIND*, January 1954) I made two mistakes as to matters of fact. The first is that I said (pp. 3-4) that Mr. R. E. Priestley was in 1930 "University Registry" ("Registrar" is a misprint for "Registrary"), whereas the office which he really held was that of Secretary General of the Faculties. The second is that (on p. 3) I stated my belief that Wittgenstein always followed the plan of giving only one lecture a week, and holding a discussion class on some other day in the same week. Professor von Wright informs me that I was mistaken in believing this: that in 1939, Wittgenstein lectured twice a week and held no discussion class; and that in the Easter term of 1947, he both gave two lectures a week and also held a discussion class. I have also remembered that at one time (I do not know for how long) he gave, besides his ordinary lectures, a special set of lectures for mathematicians.

## II.—DETERMINISTS AND LIBERTARIANS

BY P. H. NOWELL SMITH

(1)

WHAT precisely is the question at issue between determinists and libertarians? Or, better, what sort of an issue is it, and what arguments or considerations would be relevant to settling it? We must not lose sight of the fact that, like the issue between those who assert and those who deny the existence of mental images, it may be partly empirical. The moral experience of determinists and libertarians may just be different. There is no reason why this should not be so and plenty of reasons why it should. For even people who have been brought up in more or less the same moral climate and who would agree, in the main, about what was right and what was wrong might differ about the meanings of 'right' and 'wrong'.

There is no one thing that these words mean; what they mean for a given man is a complex of elements which includes the ideas, emotions and behaviour patterns that they evoke; and within this complex, which may be more or less the same for everybody, there may be great differences in the prominence of different elements. Admitting that what he has just done was wrong may mean, for one man, primarily a resolve to do better in future, for another a feeling of remorse for what he has done. This is not to say that the former would feel no remorse and latter make no resolve; but rather that they will have different conceptions of what lies at the heart of the concept of 'wrong-doing'. When they start to philosophize each will tend to say that the element that is most prominent for him is 'essential' to the concept of wrong-doing and that the other is connected with it 'only synthetically'. If either of them generalizes from his own experience and says that others must think so too, he will be mistaken.

Yet, like the dispute over images, the issue is not wholly empirical. The man who denies that images exist or insists that images are not seen is prepared to say that there is imagining and that to talk of something being 'before the mind's eye', though misleading, is not nonsensical. Those who have good visual imagery and those who do not can

certainly communicate with each other, especially if they are not philosophers; and those philosophers who, like myself, have fairly poor visual imagery can see that, reading between the lines, the views of the opposing parties are not as irreconcilable as they might seem.

So it is with the dispute over free will. Determinists have underestimated the concept of 'trying' or 'making an effort'; but they would not deny that people do try and do make efforts. What they object to is the way in which some libertarians describe what it is to try; and they object because they believe that the libertarian's account of 'trying' leads him into error. In this article I shall try to show first why the determinist objects and why, in the course of objecting, he seems to deny obvious truths, and then that the controversy rests, in part, on a mistaken assumption that is common to both parties.

One of the puzzling features of the controversy is the fact that it is, on the whole, the libertarians who insist that there is such a thing as *the* Problem of free will, while the so-called determinists tend to call this problem a pseudo-problem. (They admit, of course, that there are plenty of problems in this obscure area.) This is puzzling because, in a way, there can be no such thing as *the* Problem for the libertarian. He thinks that it is *obvious*, that we have some sort of immediate insight into the fact that men are sometimes responsible for what they do and that they would not be responsible if they were not free in some 'contra-causal' sense; and if this is so, it is obvious that men *are* free in this sense and no problem arises. Nor does he even require to use this argument; for, he says, we have an immediate and indefeasible awareness of freedom. Yet the very fact that he insists on there being a problem here would seem to show that there is something unsatisfactory about his immediate awareness.

A second puzzling feature of the dispute lies in the fact that, as often in philosophical disputes but seldom quite so blatantly as here, the disputants avow a respect for the ability of their opponents while at the same time accusing them of holding views that are not just false, but silly or absurd. The explanation is not far to seek; but to dwell on it for a moment may be instructive. The statements made by libertarians (and, indeed, by determinists), may be divided into three more or less distinct classes.

(a) There are statements made in ordinary language which, at a common sense level, seem to be obviously true, although

this is not to say that we know them to be true by immediate awareness. For example, 'People sometimes choose to do what they do, do things of their own free will, make efforts to do their duty even when it is unpleasant, are responsible for their actions, etc.' No one would think of denying these statements unless—and this is important—he thought that they implied certain philosophical statements which he has other reasons for rejecting. And we should have to examine the argument from the common sense to the philosophical statements with great care, since the presumption must be in favour of common sense. For one thing, it is hard to understand how we should have come to have the words 'choose', 'responsible', 'effort' and the phrase 'of their own free will' if these statements were false.

(b) At the other end of the scale there are statements which show themselves to be clearly philosophical by containing words and phrases that do not belong to ordinary language. Here are some examples from Professor Campbell's writings.

Man finds himself able at once to deny that his choice is adequately explained as issuing from his formed character, and at the same time to insist that it does, nevertheless, issue from his *self*.<sup>1</sup>

Formed character is an abstraction from personality—the latter involving, as indeed its most distinctive feature, the constant potency of creative activity.<sup>2</sup>

The effortful act is felt as issuing from the self, and yet not from the self regarded as just the unity of its existing conative tendencies.<sup>3</sup>

That these statements are philosophical is shown by the presence of certain words ('potency') and phrases ('issue from the self') which do not belong to ordinary language. From a common sense point of view these expressions are so strained and unnatural that anyone who used them would automatically count as philosophizing. To say this is not to condemn these statements; difficult as they are to understand, they have the merit of lacking the ambiguity and deceptive power of the third type of statement.

(c) There are statements that lie half-way between types (a) and (b) and which enable a philosopher to slide, often without his knowing it, from a non-contentious platitude to a controversial philosophical thesis. These statements sometimes betray themselves by containing words and phrases which,

<sup>1</sup> *Scepticism and Construction*, p. 115. I shall refer to this book as *S.*

<sup>2</sup> *S.*, p. 114.

<sup>3</sup> *S.*, p. 135.

although slightly unusual, do not seem to be very unnatural extensions of those used in ordinary speech.

(Man is) "free in the so-called 'popular' or 'vulgar' sense of being the arbiter between genuinely open possibilities".<sup>1</sup>  
 Everyone knows what it is to have the experience referred to 'as an effortful act of will'.<sup>2</sup>  
 Ought implies can.  
 He could have acted otherwise.

The first of these statements shows the cloven hoof in the occurrence of the word 'genuinely', which, like 'really' and 'in fact', is a sure sign of a slide from a platitude at least to a philosophical thesis and perhaps even to a paradox. And herein lies the solution to this (subordinate, but important) aspect of the problem. Any statement of type (c) can be made or denied either at a common sense or at a philosophical level. At a common sense level it is obvious that all choice is between open possibilities; I cannot choose between a peach and a pear if the possibility of taking a peach is not open to me. And for *free* choice, the possibilities must be *genuinely* open. For example, although it would be absurd to say that I didn't choose the pear, I hadn't a *free* choice if I was threatened or intimidated or too shy or too polite to take the peach. In general, I haven't a *free* choice if one of the alternatives is too silly, too inexpedient or too immoral to be taken seriously. In such cases we might even say: "I *had* to take the pear, because Jones so obviously wanted the peach; I really had no choice." And here the tell-tale 'really' shows that, of course, I did have a choice. (We also say: "I was morally obliged to tell Jones the unpleasant truth; I really had no choice"; and this is a platitude-paradox that is worth pondering.) But is the statement that all choice is between genuinely open alternatives still a platitude at the philosophical level, that is to say when it is drawn out and expanded in the way that the libertarian is going to draw it out and expand it? We shall see.

Again, the second quotation is surely right—at a common sense level. "Effortful act of will" is not a common sense phrase and perhaps this statement, as it stands, belongs to type (b). But, strained though it is, this phrase is not like 'the self'; the statement translates easily into 'Everyone knows what it is like to make an effort'; and this is true, whatever tangles we may get into when we try to describe or analyse the concept of making an effort. But, at a philo-

<sup>1</sup> S., p. 113.

<sup>2</sup> S., p. 131; cf. p. 141.



sophical level, we cannot let this pass ; for the phrase ' effortful act of will ' commits us to a certain way of describing the well-known facts that may prejudice the determinist-libertarian issue.

We can now see how the puzzling situation comes about. What happens is that Philosopher A denies, at a philosophical level, some such statement as ' Jones could have acted otherwise than he did ' and Philosopher B takes him to be denying it at a common sense level. A earns B's respect because his arguments show him to be no fool ; yet he appears to B to be denying an obvious truth. This sort of misunderstanding can hardly occur in the case of type (b) statements where the presence of special, philosophical words and phrases puts us on our guard.

As to ' ought implies can ' I shall remark only that when we concede, as we certainly do, that impossibility of performance precludes obligation, we generally have in mind such excuses as " I couldn't come because the police detained me, because of the floods, because my wife was suddenly taken ill, etc. ". We do not usually allow a person's character, desires or motives to excuse him. And this fact could be explained in either of two ways : (i) the libertarian way, which is to say that, even granted my character and motives, I *still could* have come ; or (ii) the determinist way, which is to say that the slogan ' ought implies can ' does not apply, and is never taken to apply to cases in which the possibility of doing something is precluded by the character or motives of the agent. So that, at this stage, ' ought implies can ' is a platitude that is not decisive either way.

Finally, ' A man can't be blamed if he couldn't help doing it ' is a platitude if anything is. But when ' he could have helped it ' is expanded into " quite simply, that character and circumstances being what they were, the agent could nevertheless have willed the believed higher course ",<sup>1</sup> it may be true, but cannot be a platitude. In the first place it now contains the philosophical verb ' to will ' which, unlike the adverbial ' of his own free will ' does not occur in ordinary language except in such contexts as : " I wish he would turn round. . . I'll will him to ; and she opened her eyes wide, as though to intensify the influence of her gaze. " <sup>2</sup> Secondly, the expansion is anything but " quite simple " ; it is a philosophical thesis, and the very one that the determinist is disposed to deny.

<sup>1</sup> *S.*, p. 114.

<sup>2</sup> Tolstoy, *Anna Karenina*, part v, ch. 15.

## (2)

*Introspection versus Logical Analysis.* The distinction between the common sense and the philosophical level of statement-making enables us to see that the dispute is very largely a dispute about *method*, a dispute between the method of introspection in which, confident that we know what our words mean, we proceed at once to the difficult task of describing and analysing what we find when we look within and the method of logical analysis. The determinist is acutely conscious of the fact that he is only dimly aware of what such words as 'determined', 'causal' and 'contra-causal' mean; so he cannot, as the libertarian so often does, treat the existence of contra-causal freedom as an obvious datum from which to start; nor can he agree that any statement, however platitudinous at a common sense level, is *obvious* if he is asked to put on it an interpretation that entails any statement including these difficult, metaphysical words. And here he is surely right. If anything is certain in this obscure subject, it is that the statement 'X entails a breach in causal continuity' could not be established or refuted by *introspection*.

'Introspection', as used in this context, is the name of a faculty, analogous to sight, by means of which we are aware of certain events, objects, phenomena or mental states which we are then able to report. These reports are infallible or nearly so. And there is no need to raise the question whether there is such a faculty or not (itself a very queer one) to show that libertarians *cannot* introspect some of the things they claim to introspect.

This may seem to be a presumptuous and impertinent claim. Surely the libertarian himself knows best what he finds when he introspects? This is exactly what I would deny. If I asked a friend to tell me what he saw in a room into which I could not see and he told me that he saw so many tables and chairs, it would be presumptuous and impertinent in me to doubt his word. But what if he claimed to see a piece of unpunctuality or even a couple of round squares? I could only reply that either he could not (not 'did not') see these things or that he was using very peculiar words to describe what he saw.

And it is the same with the introspective findings of philosophers. Some of them claim to know, not by analysis or argument, but immediately by introspection that their actions are uncaused. But, if 'uncaused' is not being used in

some esoteric sense, the philosopher is claiming to verify by introspection a general, negative proposition of vast and sweeping proportions. Surely 'that action was uncaused', whether true or not, could not be the report of an introspected *datum*.

To this it might be replied that the more philosophical parts of the libertarian thesis are not put forward as reports of data, but as the findings of "phenomenological analysis". But, in view of the following facts, I do not think that this reply can be sustained. (i) In answer to the objection that the contra-causal freedom he defends is a 'mere feeling', Campbell replies that it is indeed a mere feeling; the objection is not an objection since it asserts just what the thesis asserts.<sup>1</sup> The feeling of spontaneity (which we all know) is not adduced as evidence for saying that our freedom is contra-causal; nor is any argument developed from the one to the other; the assertion that a given action was uncaused or "created a definite rupture in the causal continuity of past and present" is put forward as a description of this feeling.

(ii) Again, this rupture in causal continuity is said to be "part and parcel" of what we see when we scrutinize the experience of making an effort with care.<sup>2</sup> The analogy is not with the detective who concludes that Jones was the murderer from what he sees, but with the keen observer who spots the plane in the sky when he looks carefully.

### (3)

*The immediate awareness of freedom.* Could it be by introspection that we know of the existence, at the moment of choice, of open possibilities? Do we, as it were, see these possibilities? When we talk of 'possibilities' in connexion with natural phenomena, we always say something that could be expressed by the use of the modal word 'may'. We say "it may rain or it may not"; to add "both possibilities exist" is to add a rhetorical flourish. In the case of human choice we say "I can (could) do this; but on the other hand I can (could) do that". In many cases it would be absurd, at a common sense level, to doubt that this is so or that the speaker knows it to be so; but philosophers are rightly puzzled by such platitudes and rightly ask "Just what is it that you know?" The philosopher's doubts are not allayed by the answer "What you know is that two genuinely open possibilities exist". Nor,

<sup>1</sup> *S.*, p. 138.

<sup>2</sup> *S.*, p. 131.

if he is puzzled by the question "How do I know that I can do either this or that?", is he satisfied by the answer "You know this because you 'see' that both possibilities exist". Moreover, even if it were true that he knows that he could choose either this or that by some kind of introspection, it could not be true that part of what he so knows is "and no one could predict which I shall choose". *That* he could only know by testing people's powers of prediction. (And, of course, he knows both that he can choose and to what extent people can predict *by experience*; not by inspecting his experience at the moment of choice, but in the sort of way that he knows his own name or the way home.)

Exactly the same argument applies to the interpretation of the phrase 'I could have acted otherwise'. This is something which the plain man would certainly say of many of the things he does. And, contrary to the libertarian thesis, he would say this of actions that involve neither obligation nor an effort of will. No doubt there are many important differences between making an effort to do one's duty and choosing a peach; but at the common sense level none of the phrases that we use to indicate a free choice ('open possibilities', 'alternative courses', 'could have chosen otherwise') apply to the one rather than to the other. In the last resort it is not the libertarian's belief in freedom in moral contexts, but his denial of freedom in other contexts that is paradoxical.

The issue here is again one of philosophical method; it is identical with that concerning 'open possibilities'; except that it is much clearer, because there is less temptation to construe the question as a quasi-empirical one and therefore less temptation to try to settle it by anything akin to 'looking'. If we ask whether possibilities exist (which after all, is not to stray so very far from such a common phrase as 'Is there any possibility that . . .?') we may be inclined to represent the question as analogous to 'Are there such things as tigers?' In the revised form the issue is clearly concerned with the logical role of the modal words 'can' and 'could'; and it is surely clear that *this* issue could not be settled by introspection.

To deny that 'I could have acted otherwise' is a report of an introspectable datum is not to deny the existence of the feeling of acting spontaneously to which the libertarian refers; it is to say that certain conclusions which he draws do not follow from the statement that such feelings exist, in particular conclusions about causation or the powers which others have of predicting a man's actions. And it is easy to see how the

mistake of thinking that such conclusions follow comes to be made. An expert engineer might say "I can see at a glance that that machine can't work". But of course he does not see this; he *knows* it; and he would not know it if he were not both familiar with certain general principles of engineering and also able to recognize (which itself involves more than just 'seeing') that the machine contravenes one of these principles. Only an *experienced* engineer can 'see at a glance'.

Similarly, if I have long been a martyr to indigestion, I don't just 'feel a pain in my chest'; I 'feel indigestion coming on'. But it is clear that I might be mistaken and also that I am only entitled to use the more diagnostic phrase if I have had some (perhaps in this case not much) experience of indigestion. There are situations in which the occurrence of a certain feeling is such a sure sign of the presence of a certain bodily or mental condition that we slip into saying that we feel that condition. And, from a common sense point of view, why not? It is only the philosopher who presses the words 'see' and 'feel' more heavily than they can bear who is likely to be misled.

Nor is this all; if a feeling is normally associated with a particular bodily or mental condition, we come to use the same word indifferently for the feeling and the condition. Suppose that doctors discovered a rare organic condition, different from indigestion, which was always associated with the same feeling of constriction in the chest. A man who was in this condition might still go on saying that he felt indigestion. In non-medical contexts, the word 'indigestion' might become prised off the condition and adhere to the feeling. (Compare the question: 'Has a man who has hysterical indigestion really got indigestion?')

There is an analogy here with the concept of 'understanding'. We are all familiar with the feeling of suddenly coming to understand something 'in a flash', of having 'got it'.<sup>1</sup> This feeling has the same indefeasible certainty about it that the libertarian finds in the awareness of freedom; (and also, I would add, that adheres to the *déjà-vu* experience and to the thoughts that arise as we recover from nitrous oxide gas). Is to understand Pythagoras' Theorem to have this 'got it' feeling when someone explains it? Or is it to be able to apply the theorem, to reproduce the proof and to explain, in different words, what has been proved? As often in philosophy, the tests for whether

<sup>1</sup> Cf. the discussion between A. D. Woozley and C. J. Holloway in *MIND*, vol. lvii, p. 350 and vol. lviii, p. 79.

a man understands are many ; and, as in the case of indigestion, the feeling which a man has might conflict with the diagnosis of others. In the case of 'understanding' we should surely allow the discursive, performatory test to override the test of immediate awareness. We should say that he thought he understood, but didn't really. It was quite natural for him to think that he understood ; for—and this is the point—the 'got it' feeling is a reliable, though not an infallible clue to whether one has understood or not.

Now suppose that someone were to say that just for a brief moment the man really did understand but that he could not reproduce the proof, etc., because the flash of understanding went as quickly as it came ? This is a puzzling suggestion, not because we haven't (alas !) any means of discovering whether or not he really did understand for a moment, but because we hesitate between applying the rival criteria for 'understanding'. If we decided to apply the 'flash' criterion, then, provided that we were satisfied about his honesty and the reliability of his memory, his statement that the flash occurred would be decisive. But if we try to apply the performatory test, we find that we cannot do so, since this is a test for the presence of a capacity ; and capacities neither exist nor fail to exist at a moment. The queerness of the suggestion arises from our attempting at the same time both to treat 'understanding' as a capacity and also to decide whether a man understands or not by a test appropriate to actualities. The fact that we have no use for the continuous present 'I am now understanding' is neither an accident nor a defect of language. It reveals the fact that 'understanding' is a capacity ; and a language that had no device for distinguishing capacities from actualities would be poorer, not richer. In some cases the device used is a contrast of tenses within the same verb ('is a bowler' and 'is now bowling') ; but we need verbs with *no* continuous present for those cases (like 'understanding' and 'knowing') where the capacity is not manifested or exercised in any one way. There is no one thing which corresponds to 'I understand' in the way that 'I am bowling' corresponds to 'I am a bowler'.

Similar difficulties arise whenever we try to give a purely categorical analysis of statements about capacities. (I say 'purely categorical' because a statement such as 'Jones understands *p*' is, of course, categorical as it stands.) Consider the concept of 'being an electric conductor', as opposed to that 'of conducting' or 'carrying a charge'. *Prima facie*, to

say that a certain piece of wire is a conductor is to say that it will (would) carry a charge if certain conditions are (were) fulfilled. But suppose someone were to treat it as purely categorical, that is to ask if it is a conductor unconditionally? "Would it", he asks, "still be a conductor if the conditions were not fulfilled?" This is a puzzling question because it is equivalent to the question: "Would it carry a charge, if certain conditions were fulfilled, if those conditions were not fulfilled?" and it is difficult to see what sense could be made of this question. (This question must be sharply distinguished from "Would it carry a charge if certain conditions, which are not in fact fulfilled, were fulfilled?" This last question is equivalent to "Is it a conductor although it is not now conducting?" and is by no means senseless.)

In the same way, when we ask whether someone can do or could have done something, we are asking whether he will or would have been successful if he tries or had tried to do it; and his trying to do it presupposes his wanting to do it. It is absurd to speak of success or failure or to ask whether anything prevents a man from doing something without presupposing a desire to do it on his part. And it is this presupposition that we are expressly excluding when we ask "Could he still do it, even if he did not want to?" This question makes sense if it is construed as being analogous to "Is it a conductor although it is not now conducting?" For example, I do not now want to multiply 567 by 479; I have no earthly reason for making the attempt; yet I have no doubt that I *can* do this, that is to say that if I had a reason for trying, I should try and succeed. But to interpret it to mean "Can I do it, even granted that I have no reason for trying?" is to construe it as being analogous to "Would it still be a conductor if the conditions under which it actually carries a charge are not fulfilled?"

*Prima facie*, then, statements containing 'can' and 'could' are not reports of what occurs or does not occur at a particular moment; their modality in itself suggests this,<sup>1</sup> and the onus of proof is on those who would construe them differently in the special case of being able to choose the path of duty in the face of temptation. And even if it were true that, in this special case, the modal sentence was used to report an introspected datum, it would be difficult to see how 'no one could have predicted which I would choose' or 'I created a rupture

<sup>1</sup> I have argued this point at greater length in *Ethics*, ch. 19.



in causal continuity' could be either a part of or entailed by what was reported. Moreover, if we do construe the modal sentence in this way, we ought, I think, to be more puzzled than we are by the fact that questions of responsibility, of the propriety or impropriety of blaming someone, turn on the truth or falsity of 'he could have acted otherwise'.

I should like to emphasize again the fact that I am not grinding any kind of behaviourist axe here. To say that 'I could have acted otherwise' is not a report of what one 'sees' when he looks within is not to say that it is in all cases true that a spectator knows better than a man himself what he could or could not have done if he had tried. Yet we do very often claim to override someone's statement that he couldn't help it, just as we often claim to tell him that he is angry or jealous however stoutly he denies it; and in both cases we do so without accusing him of insincerity. We suppose him to be, like the man who says he understands because he has had a flash, honestly and understandably under a misapprehension about his own powers.

Though we are often right, we may be mistaken. However good the evidence, he may be right in claiming that he couldn't help it, just as he might be right in claiming that he was not really angry (but not, I think, in claiming that he understood), in a case where the public evidence was against him. But it does not follow that he is right because he, and he alone is in a position to 'see' the occurrence or state of affairs of which 'I couldn't help it' is a report. It may be because he, and he alone is in possession of certain facts which have a bearing on the truth of this statement. One may admit the existence of 'private' thoughts and feelings and admit that they may be relevant to questions about a man's capacity without conceding that 'I couldn't help it' is a report of such a feeling. The spectator is led astray because he lacks certain vital clues which the agent possesses; and he may lack these clues because the agent may be unable, however willing, to disclose his state of mind. In this case 'I can never really know what goes on in the mind of another' is not, as some philosophers have supposed, an absurdity based on a category-confusion; nor is it, as others have supposed, a logical truism. It is a statement of an empirical fact which is due to the inadequacy of the language that we use for making personal disclosures, an inadequacy which is to be measured, not against some unattainable standard of cast-iron logical certainty, but against the standard of a language that might be used by people who understood each other when

they described their thoughts, motives and feelings as well as they understand each other when they talk about tables and chairs or about addition and subtraction. Why our common language for talking about thoughts, motives and feelings should be so much less adequate than our common mathematical language is a question which, no doubt, admits of an answer. But I do not know the answer; I suspect that it would be difficult to discover it; and, for my present purpose, it is the fact and not the explanation that is relevant. When we say to someone 'You really could have helped it' without impugning his honesty, we are sometimes right and sometimes wrong. If the introspectionist theory were correct, we should always be wrong.

## (4)

*The Self and the Character.* It is an old criticism of libertarianism that it makes out voluntary and deliberate actions to be 'chance events', and libertarians are as anxious to deny this 'indeterminist' theory as they are to deny determinism. In this section I shall try to show that the conception of actions which "issue from the self, and yet not from the self regarded as just the unity of its existing conative tendencies"<sup>1</sup> cannot be sustained. It must turn out either to be the repudiated doctrine of indeterminism or to be a doctrine in which the 'self' is identified with the whole or a part of what the determinist calls the 'character'.

Here we must notice again that the controversy cannot be settled by introspection. "But it is equally evident that the effort is felt not to be determined by the self's existent conative dispositions (or, as we may call it, its 'character as so far formed')." <sup>2</sup> This may be *true*; but it cannot be *evident*. The phrase 'conative disposition' is not a phrase of ordinary language; it is embedded in a large and complex mass of psychological theory, which may well be false, or at least misleading; and to use it in a report of what one 'sees' is to commit oneself to the language and concepts of this theory. And 'determined' takes us well beyond even psychology into metaphysics. To say this is not to condemn it; maybe metaphysics is just what is needed here. But a metaphysician is not a reporter; he is an interpreter of what he 'sees'; and it is over the interpretation that philosophical disputes arise.

Moreover the phrase 'the self' (as opposed to 'himself', 'myself', etc.) is not a phrase of ordinary language; and if it is to be introduced, rules for its use must be specified. In particular we must be told what 'self-determined' means and what the criteria of identity ('the same self') are. For example, does 'that was an act of Jones' self' mean more than 'Jones did it'? It is difficult to see what more it could mean; yet it must mean more, since those things which Jones does, other than the acts of Jones' self, are said by the libertarian to be determined.

Suppose we try to construe 'self-determined' by analogy with other 'self-' compounds, self-adjusting, self-governing, self-centred, etc.? This will not do; for in none of these cases is there any reference to any special part of the object concerned called the 'self'. For example, the self-centred man does not continually think and talk about his 'self', but about *himself*, his actions, desires, motives, exploits, etc., as opposed to other people's. And if 'self-determined' is construed in this way, the theory differs not at all from determinism. For a 'self-determined action' will be, not an action determined by 'the self', but an action determined by some motive or characteristic of the agent; and, in one form or another, that is just what the determinist asserts.

Perhaps it is wrong to try to think of 'the self' as just a 'part' of the agent. It is such a *special* part that we ought, perhaps, to call it his 'substance' (characteristic traits, intellectual and physical capacities, etc.; being mere attributes); but, if it is a substance, what are the criteria of its identity? Is there any way of deciding whether two or more acts are acts of the same self except by showing that they display some sort of consistency or continuity of attributes, that they are 'in character'. The 'self' appears to vanish either in the 'character', as the determinist asserts, or into a series of *disconnected* interventions, interruptions into the 'causal chain', which is what the indeterminists (if there are any) assert.

(5)

*The Concept of Forces.* So far I seem to have been defending a form of determinism; but this is not the case. I have simply refrained from raising the familiar objections to determinism, which are just as fatal. For the fact is that 'determined' and 'contra-causal' have not been analysed and until their

meanings are made clear, until we know to what we are committed when we say that choice is or is not contra-causal, the disputants must be fighting in the dark. There are at least three different bogeys that go under the name of 'determinism' and they must be kept distinct because each requires a different technique of exorcism.

(i) There is logical determinism or fatalism, according to which we cannot help doing what we do since things are what they are and our actions will be what they will be. I shall forbear to comment on this brand of determinism, since I doubt whether anyone is now seriously deceived by it.

(ii) There is the theory that we might call 'literal mechanical determinism'. Every human action, no matter what else it may be, is a complex but determinate movement of a determinate body. Suppose that this body were, quite literally, a complex machine? Suppose that we could predict its every movement in accordance with the laws of classical mechanics if only we knew enough about the particles concerned? This is the eighteenth-century bogey, and about it one can say little except that it might be true, that there is nothing in the present trend of natural science to warrant it (indeed it seems to presuppose a misunderstanding of explanation and prediction even within natural science) and that, if it were true, our talk about human conduct would be quite different from what it actually is.

(iii) There is also the obscure, elusive and much more important theory that desires (and other motives) are somehow *like* mechanical forces. The mechanical analogy is recognized as an analogy, but accepted without criticism as a good one. The idea seems to be that a man (or a 'self') is like a billiard-ball on which certain forces (his desires) act. The determinists say that all his actions are determined by the strength and direction of the forces, as movements are in classical mechanics, so that he 'must' do what he does in the same sense that a billiard ball 'must' move as it does. The libertarian agrees that this is true in the case of most human actions but wants to make a special exception in the case of those situations that involve a conflict between desires and the sense of duty. The interplay of the forces, he says, is liable to be upset by the operations of a special sort of force, to which there is no analogue in mechanics, called "will-energy". Of this force little can be said except that we all feel it within us (and that is why little needs to be said), that we can use it or fail to use it, that it is unpredictable in its operations and that our status as free, responsible beings depends on its existence. Just why an

observer is supposed to be able to discover the exact nature of the other forces but not of "will-energy" is not made clear; but this must be the case, since it is held that all actions other than those involving the sense of duty are, in principle, predictable in a para-mechanical way.

Now this bald statement of the analogy is, of course, a travesty. People who adopt this theory, or rather people who adopt this way of talking would repudiate the absurd notion that a desire is a force or that it is something inside a man which exerts a force on him. They would be indignant if you asked them how the force is measured or to point to the thing that exerts it. But the underlying conception creeps in more insidiously in the use of such words and phrases as 'impulse', 'inclination', 'line of least resistance', 'reinforce the higher but weaker desire', 'my character inclines me', 'letting my desiring nature have its way'. In the last example the mechanical analogy merges into another well-known analogy, the political. 'My desiring nature' is thought of as another person, different from my 'self' who attempts to tyrannize over me, to whom I may be a slave.

These analogies are not the inventions of philosophers; they are deeply embedded in our talk about choice and conduct and I shall try to show later why this is so. For the moment I want to show that, so long as the dispute is conducted in these terms the solution must be, not that either party is right, but that they make a common mistake in treating the analogy too uncritically. The dispute seems to be about the existence of an 'area' within which free will 'operates'. It is agreed that men are machines; the question is whether there is any play in the machine.

One example will suffice to illustrate what happens if the dispute is conducted in these terms. Here is a part of McDougall's description of what it is to make a moral effort.

Some attempt must therefore be made to show that the effort of volition . . . involves no new principles of activity and energy, but only a more subtle and complex interplay of those impulses which actuate all animal behaviour. . . . The source of that *influx of vital energy* which seems to play the decisive role in volition. . . . The conations, the desires and aversions, arising within this self-regarding sentiment, are the *motive forces* which, *adding* themselves to the weaker ideal motive in the case of moral effort, enable it to win the *mastery* over some stronger, coarser desire of our primitive animal nature and to *banish* from consciousness the idea of the end of this desire.<sup>1</sup>

<sup>1</sup> Quoted by Campbell, *S.*, pp. 146-147 (my italics).

Here, of course, McDougall makes the additional mistake of supposing that all desires, other than the desire to do one's duty, are self-regarding, a mistake that I shall ignore in spite of the fact that it may lie at the heart of the problem. We are concerned at the moment with the question whether this is an adequate account of making a moral effort. Campbell complains, quite rightly, that it is not; it fails to solve the problem which it set out to solve of explaining how a man can 'take the line of greatest resistance', because it amounts to saying that, in the end, when we take *all* the forces into account, he always takes the line of least resistance.

But the alternative account is no more satisfactory. The self, it is said, intervenes by putting forth will-energy. But, if will-energy is just another force, the agent takes the line of least resistance after all, exactly as in McDougall's account, and we are back in the quagmire of determinism. And if it is not just another force, how can it be said to *reinforce* the higher, but weaker desire, to oppose, sometimes to overcome and sometimes to be overcome by the other forces? If the mechanical analogy is correct, it would seem to be axiomatic that anything which opposes or reinforces a force must be another force.

Moreover, if the question of a man's responsibility turns on the use that he makes of his stock of will-energy, it would seem necessary to ask whether he could, on a given occasion, have made more use of it than he did; and this will introduce an infinite regress into the analysis. His ability to have *acted* rightly was said to turn on his possession of sufficient will-energy; and his ability to have *made more use* of this will-energy will presumably turn on his possession of sufficient will-energy of a second order.

(6)

*Character and Conduct.* Another objection which the libertarian takes most seriously is that his theory "posits a breach of causal continuity between a man's character and his conduct" and that it does not allow for the obvious fact that a man's behaviour is often, within broad limits, predictable. To this it is replied that much of a man's behaviour is indeed predictable, because it is determined (apparently in a more or less mechanical way) by his tastes and interests. "Action, although free, will be limited to those courses of action which the interests, guided by the intelligence, of the agent suggest

to him as possible modes of self-satisfaction. Freedom exists only within this prescribed area. . . . Knowing within broad limits what kind of interests a person has, we are entitled to predict, within broad limits, what kind of response he will make to a practical stimulus."<sup>1</sup>

This last observation seems obviously true, but equally obviously to apply to moral as well as to non-moral choices. It is only within broad limits that we can predict what Jones will choose from a menu, whether he will make a century to-day or whether he will finish his cross-word puzzle; and, on the other side, we *can*, again within broad limits, predict whether he will overcome the temptation to run away from danger or cheat at cards. There seems to be no crucial difference in principle between the reliance that we place on the accuracy of a bank clerk and the reliance we place on his integrity. The objection is not that the libertarian does not allow us *enough* predictability or continuity of character and cannot be met by appealing to the *amount* he does allow us.

The basic objection lies, as I said, not so much to the libertarian's refusal to accept mechanical or para-mechanical determinism as an account of moral choice but to the ease with which he accepts it as an account of non-moral choice. Now it is true that we often say that a man's physical and intellectual powers, tastes, interests and habits are 'due to' or 'caused by' hereditary or environmental factors; but we do not, unless we are in the grip of a metaphysical theory, think of these factors as antecedent causes of a mechanical kind. One man inherits musical ability from his father; a second acquires a taste for Homer from an inspiring teacher; a third takes to gardening because he is crossed in love. In each case we give an explanation or, if you like, 'assign a cause'. But we do not, as libertarians seem to suppose, assign a mechanical cause. No doubt explanation of conduct is trivially like mechanical explanation in some ways; for example, they are both inductive. But a story about motives, desires and character is not, as it stands, a story about particles; nor is it altogether like one. It may be true, as the second type of determinist supposes, that every action that we explain in terms of motives could also be explained in terms of forces and particles; but it does not follow that the one explanation is, item for item, a reflection of the other. And, if there is no reason to regard explanation

<sup>1</sup> *S.*, p. 163. Notice that the analogy is still with a machine, but with a machine in which there is a certain amount of play.



and prediction of non-moral choices as mechanical or para-mechanical, one of the reasons for making an exception in the case of moral choices disappears.

Another concept that requires analysis in this connexion is that of 'impossibility'. For the dispute about the existence of an area within which free will operates re-emerges in the question: 'Is it impossible or only unlikely that a man who has acquired bad habits will break with his past? Is it impossible or only unlikely that a man who is known to be of a cowardly disposition will stand his ground here and now?'

"The libertarian is not compelled by his theory to ignore the enslaving chains of bad habits. He will differ from the determinist in his attitude to what look like notable discrepancies between formed character and conduct only in that, while for him they are *unlikely*, for the determinist they are *impossible*."<sup>1</sup> It looks as if the determinist is here being made to say that bad habits or formed character are antecedent causes of conduct. But the only sense in which character could be said to be a 'cause' of conduct is that of formal cause. The connexion between character and conduct is indeed a necessary one; but it is a logical and not a causal connexion. If Jones is dishonest, he *must* tell lies, forge cheques, steal or cheat at cards; but this is not because either his character or his past actions compel him to do one or other of these things on a certain occasion. It is because regular abstention from such practices is incompatible with the character-predicate 'dishonest'. It is only in this sense that it is 'impossible' for him not to steal if he is dishonest. And even this logical connexion between conduct and character admits of exceptions, provided that they are rare, since statements about character are statements about how someone usually behaves and are not refuted by single contrary instances. The determinist differs from the libertarian, not in making such discrepancies impossible, but in refusing to attach much importance to them, in hoping rather than fearing that the logical connexion may one day be tightened up and, above all, in refusing to make the existence of the discrepancies a necessary condition of responsibility.

For the rigid distinction between 'formed character', where strict determinism reigns, and 'creative activity', it would be better to substitute a conception of continual modification of character, in its moral as well as its other aspects, which is not only, I think, nearer both to the observed facts and to our

<sup>1</sup> S., p. 164.

ordinary talk about character, but also avoids the sharp separation between moral and non-moral choice that the libertarian theory implies. A man can become more or less conscientious, just as he can become more or less good at tennis or fond of Mozart.

## (7)

Finally, having pleaded for the rejection of the mechanical analogy, I shall try to show just what makes this analogy plausible and also to bring out what I believe to be the truth underlying the distinction between 'the self' and those parts of our personality and behaviour that we refuse to identify with 'ourselves'. No doubt there are deep psychological causes for our exploiting this distinction, for example a desire to repudiate or disown actions and traits of character of which we are ashamed. But these, though important, can hardly be what gives the conception of desires as alien, external or hostile forces its initial plausibility. Briefly the answer seems to be this.

There are desires which we are inclined to think of as external forces or malevolent tyrants because they do, like real physical forces and real tyrants, prevent us doing what we want to do. A swimmer has to battle against a strong tide, and the tide may be too strong for him. In the same way we have to battle against our cravings. The craving that is, perhaps, most familiar to most of us is the craving for tobacco; in any case it will do as an example. I very much want to smoke; I know that if I do so, certain evil consequences will occur, for example that I shall undermine my health or not have enough money to pay for my children's education. And this conflict of ends is radically unlike the situation in which I simply *want* to do two incompatible things, however much I may want to do them. For the difference lies not in the greater strength of the craving, but in the fact that it conflicts with my general conception of what is best for me, of the sort of life that I ought to lead. Other motives, however powerful, do not for the most part give rise to such a conflict. Sympathy, parental affection and fondness for Italian opera are examples. Even these can, of course, become 'ruling passions'; but in general they are not so. And in this connexion we must be on our guard against the fatal philosophical habit of referring to all motives (other than the sense of duty) as 'desires', a habit which is the first step towards thinking of them as cravings, forces or tyrants.

It is not easy to elucidate this contrast between those 'desires' which can and those which cannot be appropriately likened to

'external forces or hostile beings ; and I shall make no attempt to elucidate it here. Four things, however, may be said about it. (i) The force and tyrant metaphors, in which a man's desires are represented as being external to 'himself', lose their point if they are extended to cover *all* desires. For these metaphors presuppose that the alien desires prevent or tend to prevent a man doing what he really wants to do ; and this in turn presupposes that there is something that he really wants to do. (ii) There is no reason to equate a man's *existing conative tendencies* with those (few) desires that can plausibly be thought of as hostile forces. (iii) The contrast, if valid, gives no grounds for supposing that a man's capacity to make moral efforts is any more or less a part of his 'formed character' than his proneness to indulge this or that desire. (iv) The contrast has nothing to do with the predictability of conduct ; and it is this rather than any dispute about the mechanics of conduct that is the crux of the libertarian-determinist controversy. Conscientiousness and its opposite are no doubt very important and in some ways peculiar and puzzling characteristics ; but they are characteristics none the less.

*University of Oxford*

### III.—CONCERNING UNDEFINED DESCRIPTIVE PREDICATES OF HIGHER LEVELS<sup>1</sup>

BY JULIUS R. WEINBERG

WHEN we consider the way in which individual signs and constant descriptive predicates function in our discourse, it seems quite natural to say that they *designate* extra-discursive objects in some unambiguous sense of the term *designate*. The notational similarity between  $F(f)$  and  $g(a)$  encourages us to suppose that second and higher level predicates also stand in some designation-relation to extra-discursive objects. I believe this to be an illusion which arises from some faulty views about abstraction and from the misleading features of our notational abbreviations.

The best way I know of to proceed with an investigation of this kind is to begin with defined descriptive predicates of higher level and to show that we can and should dispense with all talk about designation. This will establish presumptive evidence against undefined descriptive predicates of higher level. We can then proceed to ask whether there are any more conclusive arguments against such predicates, and I believe that we shall find them.

That there must be undefined descriptive predicates of the lowest level for predicates cannot be successfully gainsaid. Some philosophers have, in the interest of a sort of nominalism, attempted explicitly to supplant such predicates by resemblance relations. But it is clear that the attempts in this direction come short. For firstly, one resemblance relation will not do; we must have as many as there are abstraction-classes to be formed. Secondly, we cannot choose arbitrary objects as standards and form abstraction classes in terms of all objects which resemble the selected object, because there are no permanent objects to serve this purpose. Finally, while there may be a higher level predicate *resemblance-relation* it is pretty plain

<sup>1</sup> This essay owes very much, as will be apparent, to the late F. P. Ramsay's "Universals", which is reprinted in the *Foundations of Mathematics*, London, 1931. Its immediate stimulus, however, has been some of the essays of Gustav Bergmann, particularly his "Logical Atomism, Elementarism, and the Analysis of Values", *Philosophical Studies*, vol. ii, no. 6, December 1951.

that this sort of predicate is definable and thus can be shown to pre-suppose an undefined relation or predicate of the sort just mentioned. Now since there is no economy in starting with undefined dyadic predicates instead of undefined monadic predicates, we may as well introduce the latter.

Yet we should at once guard against a misunderstanding. Just because we cannot dispense with undefined descriptive predicates it is sometimes supposed that we are involved in a kind of platonic realism applied to these predicates, *i.e.* that these predicates designate in the same way in which individual names denote. This is, I believe, a mistake. Undefined descriptive first level predicates are classifying symbols or relating symbols and their symbolic function is quite distinct from that of individual names. But it is also true that neither names without predicates nor predicates without names or bound variables can occur significantly. The smallest unit of significant discourse is a complete sentence, and, in order to form a complete sentence we must employ names of things (or variables) and predicates. We might suppose that we can speak of the referent of the predicate in isolation from such sentences, *e.g.* when we say  $(\exists g)g = \text{red}$ , and I suppose that there is the temptation to read such a sentence as "There is at least one property identical with red". But if we ask ourselves how we come to write such sentences we discover the following. First, assuming that " $=$ " is defined in our logic, such a sentence means " $(\exists g)F(g) \supset F(\text{red})$ ", second, that *red* only occurs in such a context with its arguments, and third, that unless we have at least one true sentence of the form *red a* we cannot assign any meaning to *red* at all. Hence the temptation to use *red* in sentences which isolate that predicate from its arguments must be resisted. To put the point in another way, because a constant predicate can occur with several distinct arguments or with apparent variables does not mean that it has any significance in isolation. We do not suppose that purely indexical symbols, *i.e.* logically proper names can significantly occur in such isolation from qualitative or relational predicates, and there is really no ground for supposing that the latter can occur without other symbols as arguments. The fact seems to be that, at the lowest level of significant discourse, we encounter two different sorts of symbols, predicates and names (or bound variables), and that neither can function symbolically without the other.

Yet it makes some sense to say that *red* classifies what *this* indicates in such a sentence as "This is red". That is to say,

between such symbols and the extra-discursive world which symbols are used to describe, there is some direct relation. The question which interests us now is whether we can legitimately extend this to higher level predicates, and, in the case of defined predicates of higher level, at least, it seems that we cannot legitimately do so.

Let us consider a definition of a higher level predicate in order to see this. Put " $1(F)$ " for " $(\exists x)[Fx \cdot \sim(\exists y)y \neq x \cdot Fy]$ ". Our question might now be put as follows: Should we say that " $1$ " designates what is designated by

$$"(\exists x)[Fx \cdot \sim(\exists y)y \neq x \cdot Fy]"?$$

Since " $1$ " is simply an abbreviation for a prolix formula consisting entirely of logical constants, and since logical constants function symbolically in a non-designatory way (cf. Wittgenstein "The logical constants do not represent", but also the scholastics on syncategorematic terms), I am sure we should not say such a thing. The temptation to do so may arise from the erroneous "Fido"—Fido principle which Ryle has again reminded us of. Some appear to feel that a symbol either designates or is meaningless, but the absurdity of searching for referents of "or", "and", "not", and "none" ought to provide the moral stamina required to resist temptation of this sort.

It is true that we can use our abbreviations in ways that suggest that they designate something extra-discursive. For, as is well-known, if  $\text{El}(\cdot x)(gx)$  then  $(x)fx \supset f(\cdot x)(gx)$ , i.e. if "the so-and-so exists" is true then any predicate of everything is a predicate of the so-and-so. In other words, if a descriptive phrase describes something, then it functions discursively just as a proper-name functions. The same is true when we are dealing with defined predicates of higher level. But this, again, is due to the logical principles and in no way requires an extra-discursive referent for such expressions.

Let us summarize the results so far. We have seen that a number of considerations suggest that predicates of higher level designate something extra-discursive just as the undefined descriptive predicates of lowest level do. Yet such predicates (in abbreviated form) are mere abbreviations for formulae containing no descriptive constants. And because logical constants do not serve representatively at all, there can be no designation-relation between such symbols and any extra-discursive object.

We shall now turn our attention to *undefined* descriptive predicates of higher level. Let us select some candidates for

such predicates. We shall then see why the suggestion that there are such predicates is so very unpalatable. Consider, first of all, "red is a colour". If colour be a second level undefined descriptive predicate, the symbolic form of such a proposition will be "colour (red)". We must attempt to show (1) that "red" cannot occur as argument to such a function as "colour" taken as undefined, and (2) that "colour" can be defined, and, finally, (3) that only then can we accept "colour (red)" as a satisfactory expression.

The way in which "red" occurs in such contexts as "This is red", "Something is red", etc., precludes the possibility that red can occur in isolation from such contexts as it must if it is to be argument of a higher level undefined descriptive predicate. In such an expression, the fact that "red" can function only in such contexts as we have mentioned has been implicitly rejected. Yet "red" insofar as it serves these other functions cannot suddenly acquire an independence which its previously determined uses make impossible. It is sometimes supposed that a principle of extensionality has been included among the rules, and that it is such an arbitrarily assumed principle which is responsible for the difficulty. Yet such an argument is a *hysteron proteron*. The extensionality principle is introduced by a philosophical logician because the limitations imposed by the obvious, primary, and indispensable function of certain symbols appear to preclude certain other functions of such symbols. If, that is to say, "red" is used in sentences which represent objects named or classified descriptively in a constant way, "red" cannot be used as if it were the name of something which could be classified just as individuals can be classified by descriptive predicates. That we could devise "redness" for the purpose is true. But the connexion of "redness" with "red" is then the problem. The bitter memories of the one and the many paradox will discourage some of us from introducing "redness". It might be suggested, of course, that "red" and other first level predicates be introduced originally as names of constant properties and that "red a" means that the individual a stands in some objective relation to *redness*. But it is clear that this suggestion won't do. For first of all the relational predicate, let us call it *having*, would, by a parity of reasoning, have to be introduced as a name, and if this too has a kind of linguistic independence, I do not see how the process can terminate. In the second place, the suggestion also plainly assumes that there are individuals which have but which might not have characteristics, i.e. it allows  $(\exists x) \sim (\exists y) gx$



as logically possible. Yet reflection about the purely indicative function of such symbols as individual names should show that we cannot have substantives of this sort. The result is, I believe, that allowing predicates or names to stand alone is an illegitimate separation of symbols from the contexts in which alone they contribute to forming significant statements. Put very crudely, there is that which is red and there is that which is red and round, but it makes no sense to say that there are things which have red and round. This terminology in fact is a survival of the inference from the truth that sentences must have subjects to the existence of *materia prima* as the substratum of qualities.

Apart from this difficulty about the double-life which "red" has to lead if it is to occur both in "this is red" and "red is a colour" (colour undefined), we can also ask how we might verify statements in which undefined descriptive predicates of higher level occur. It was, traditionally, customary to say that we obtained cognition of the referents of abstract terms such as "colour" by a sort of intellectual or non-perceptual intuition. There is no way I know of for definitely refuting the claim that there are such intuitions, and as Professor Church says, we cannot legislate for the experience of others. But there are two considerations against this suggestion. First, the plausibility of such intuitions rests in part on the fact that, by proper direction and orientation we can get other people to feel that they too are having the intuition in question (*cf.* Plato's dialectical method of getting people to "understand" what certain abstract terms mean). Yet we know that ability to use words correctly is a far cry from elucidating their symbolic function so that we may go beyond such vague appeals to agreement. And secondly, we know that attempts to give explicit definitions of such allegedly indefinable abstractions have succeeded in a sufficiently remarkable number of instances to suggest that non-perceptual intuition is a last if not a desperate resort.

Moreover, "colour" can be defined in terms of relations of individuals to which "red", "green", etc. are properly applicable. Or it can be defined in terms of the predicate "red" etc. and relations of individuals which such predicates classify. And, as we have seen, there is no difficulty with defined descriptive predicates of higher level.

The objector might say that there is, after all, a considerable difference between the defined logical predicates of P.M., and the defined descriptive predicates of a system which employs P.M. symbolism and descriptive predicates. This is, of course, quite

true but it does not affect the present issue. For if we examine such systems (e.g. Russell's *On Order in Time*) what we find is this. Some undefined descriptive predicate, e.g. *preceding* (*temporal*) is selected as basic and all other predicates are introduced explicitly by defining them in terms of the undefined predicates and logical constants. The significance of such defined descriptive predicates does depend, it is true, on symbols which have an unambiguous and direct designating function. This does not, however, mean that these defined predicates have such a direct and unambiguous relation to extra-discursive objects. That something is red or blue is a compendious way of saying that there is that which is red or which is blue. But there is no single referent of *red or blue*. The principle involved in more complicated cases is not essentially different. And from the facts that *red* symbolizes directly and that *blue* symbolizes directly it does not follow that *red or blue* does.

The question has been recently discussed from the following point of view: When we quantify as in the proposition  $(\exists x)gx . xRa . fa$  are we asserting that there is an individual such that etc. or are we asserting that there is a constant such that etc.? The first would "commit us to *individuals*"; the second is a metamathematical reading which, while correct, presupposes another reading which is in the object-language. The correct answer, I believe, is that such an expression as  $(\exists x)gx . xRa . fa$  must always be read as "There is that which . . .", and that this is to be understood neither as *referring* to unnamed individuals nor as *referring* to unavailable names. The operand of a quantifier and its repeated expression in the matrix are not separable parts of the symbolism. We use such expressions, e.g. when we wish to assert propositions whose truth we are not yet (or perhaps ever) in the position to ascertain, but whose meaning we know. Hence we are not asserting that there is a name which applies to something not yet available for inspection nor that there is something to which we might apply a label in our possession. We are asserting that there is that which is *g* etc. and the expression "that which is *g* etc." and not less than that whole expression is what is being asserted. All talk about *individuals* inevitably suggests that we can separate the indexical from the predicate symbols, and it is this which I am especially concerned to deny.

It is the same when we quantify predicates. The expression  $(\exists g)ga . g \neq f . fa$  is not to be understood as meaning that "this individual which has *f* has also some other property" but simply "There is that which this is and this is *f* etc."

Again the separation of *that which this is* into parts capable of referring in mutual isolation from one another is not to be countenanced. It is convenient, of course, to use other forms of speech, and there is no objection to speaking of *properties* and *relations* as long as we recognize these as mere conveniences.

Now I should like to suggest that our proposed rejection of undefined descriptive predicates of higher level is connected with the theory of logical types.

It is well known that the theory of logical types cannot be formulated in terms of a hierarchy of objects without reintroducing paradoxes which the theory was expressly designed to avoid. The only alternative, then, was to formulate the theory in terms of a hierarchy of symbols. As such it appears to be a mere heuristic device. Yet I think that the foregoing remarks render the theory less arbitrary than it appears at first sight to be.

When we attempt to produce the paradox which prompted the formulation of the theory of types we discover the following: We put  $K(g) \equiv \sim g(g)$  and obtain  $K(K) \equiv \sim K(K)$  which is a contradiction. Now as Wittgenstein observed the expression " $\sim g(g)$ " is not correctly formed since the outer and inner occurrence of " $g$ " appear to be the same only because we have neglected to write out the inner " $g$ " completely, i.e. as " $gx$ ". When we do so we see why  $gx$  cannot take  $g$  as an argument. As I have put it above, this amounts to, e.g. " $red$ " functioning both as a classifier as in " $this is red$ " and as a name as in " $red is colour$ " (" $colour$ " taken as undefined). And as we saw, this cannot be done legitimately. And we can also see now why the theory of types cannot be regarded as a hierarchical ordering of *objects*, but only as of symbols. For, the higher type symbols which we thoroughly understand (the defined ones) do not represent objects at all. It is even senseless to suppose that they do. In other words, the theory of types appears to be an arbitrary prohibition to speak of things, properties of things, etc. in the same breath. But, if the present account is correct, the basis of this prohibition is explained. For if there is no significant unambiguous sense in which names, first level, and higher level predicates all "*designate*", it would be quite unreasonable to attempt to find any designata for higher level predicates at all; and also unreasonable to expect that first level predicates have designata that are comparable with the designata of names.

*University of Wisconsin*

#### IV.—THE CRITERIA OF ASSENT TO A MORAL RULE

By H. J. N. HORSBURGH

##### I

IN his recent book, *The Language of Morals*, Mr. Hare emphasises anew the problem of Akrasia which has been a recognised source of difficulty to philosophers since the time of Plato. Part of the problem is how to escape between the horns of opposing absurdities. On the one hand, one must avoid the Socratic paradox of maintaining that everyone always does what he thinks he ought; and, on the other hand, one must discountenance the devil's paradox according to which a man may be held to assent to a moral rule to which he only pays the courtesy of verbal acceptance. According to Mr. Hare the trouble arises because our criteria in ordinary speech for saying 'he thinks he ought . . .' are very elastic. Thus, even if a person fails to fulfil an obligation we are normally prepared to say that he has not done what he thinks he ought if the omission is accompanied by remorse or feelings of guilt. It is therefore necessary to admit that there are degrees of assent not all of which involve the actual fulfilment of an obligation.

I agree with Mr. Hare that the elasticity of our criteria is a source of difficulty, and part of what I wish to do in this paper is to justify Mr. Hare's use of the word 'elasticity' in this connexion. But there is also a tendency for us to be misled by language into the adoption of unsatisfactory views with regard to the nature and interrelations of our criteria; and therefore, I shall begin by examining a group of theories which seem to illustrate this tendency. (It is not implied that any of these theories is held by a reputable philosopher.) The conclusions of this section will be largely negative. It should serve to clarify the requirements of a satisfactory theory, however. I shall then consider what is the criterion of full acceptance. Next I shall attempt to justify the view that our less exacting criteria are derived from this strict criterion by a process of stretching or dilution. I shall then enquire whether the theory that I have been developing is able to cope with various difficulties that emerge in Section II. And finally, I shall try to relate what I have said to the question of weakness of the will.

## II

Mr. Hare's account of the matter suggests, although this may not be his view, that obedience to a moral rule and feeling remorse when one has failed to conform with it are both criteria of assent to the moral rule. There may also be other criteria; *e.g.* having feelings of guilt when one has broken a moral rule and being tempted to break a moral rule (for temptation implies resistance to breaking the rule, though not necessarily sufficient resistance to refrain from breaking it). This seems a plausible view. After all, if one were asked what leads one to suppose that A assents to a moral rule, on one occasion one might say, "Because he conforms with it", and on another, "Because he's remorseful when he fails to conform with it". This suggests that one uses conformity as a standard at one time and remorse as a standard at another.

It seems that a similar multiplicity of criteria can be detected with regard to some of our uses of the verb 'to know'. Suppose that A asks B whether he knows the more celebrated of Eisenhower's Christian names. B says that he does but that he cannot at the moment recall what it is. "I know it", he declares, "but for the time it escapes me. But if you presented me with a list of names which included it I'd be able to pick it out at once." A now recites a list of names: "Franklin, Elmer, Willard, Dwight—." "Yes", B interrupts him, "that's right, it's Dwight". In such a case, although B has failed to recall the name he claims to know we should certainly accept his claim on the ground that he instantly recognised it when he heard it spoken.

The cases in which we say that a man accepts a moral rule to which, on some particular occasion, he fails to conform, may be thought to be similar to the cases in which we allow that someone knows a certain name, although, on some particular occasion, he is unable to recall it. In both sorts of situation, it might be said, failure to satisfy one criterion is offset by the capacity to satisfy another. Thus, in the cases of failure to conform with a moral rule, although the obedience criterion of assent is unsatisfied, the criteria of remorse, feelings of guilt and repentance may all be satisfied—and the satisfaction of these criteria is sufficient to dispose us to say that we are concerned with a case of assent to a moral rule.

But if we have several independent criteria for saying 'he thinks he ought . . .' surely that phrase must be exceedingly ambiguous? Yet, although we are prepared to concede that

it is vague, I think that on reflection we are disposed to deny that it is ambiguous in the sense implied by the alleged plurality of independent criteria. It would seem, therefore, that our criteria do not operate with the independence attributed to them on this view.

An alternative theory can be conveniently considered by first returning to the criteria of recognition and recall. Suppose that B is never able to recall Eisenhower's Christian name although he invariably recognises the name 'Dwight' when he is presented with it, would we still say that he knows Eisenhower's Christian name? I think we might because of the triviality of the claim. On the other hand, if B claimed that he knew the correct translations into French of a number of English words but was unable to do more than recognise the correct translations when presented with them, we should be more grudging. We might say, "You don't really know the French for these words", or, "You only half-know these French words". By these qualifications we signify that while we are sometimes prepared to regard recognition as a sufficient criterion of knowledge this is in the nature of a concession which we are not prepared to make when faced with important claims to knowledge. Such claims can be made good only by satisfying both criteria. Now, it should be noted that these criteria are logically independent in the sense that there is no logical connexion between the satisfaction or non-satisfaction of one criterion and the satisfaction or non-satisfaction of the other. In view of this it might seem strange that the fact that there are two criteria for the use of the verb 'to know' does not give rise to ambiguity. The most vital protection against ambiguity has already been mentioned, namely, that the verb 'to know' is not countenanced in any important context unless both criteria can be satisfied. But the verb 'to know' is protected in two further ways. The first of these is that the combined criterion can always be insisted upon because it is always possible to apply both criteria. This also potentially reduces the different kinds of cases that have to be allowed for to those in which both criteria are satisfied, those in which one criterion is satisfied, and those in which neither criterion is satisfied. A fortunate empirical consideration simplifies the situation further. I have in mind the fact that those who are able to satisfy the criterion of recall can almost invariably satisfy the criterion of recognition. Satisfying the former criterion, therefore, is like passing one part of an examination with such high honours that one secures exemption from the rest of the examination. The cases in

which one criterion is satisfied are therefore all of one sort, namely, cases in which the criterion of recall is unsatisfied. For all these reasons, then, the multiplicity of criteria for the use of the verb 'to know' very seldom gives rise to ambiguity even when trivial matters are under discussion.

Perhaps we avoid ambiguity analogously in our use of the phrase 'he thinks he ought . . .'. In other words, may we not say that on important occasions we insist on the satisfaction of all our criteria of assent to a moral rule? But such insistence is only possible if our criteria are logically independent in the sense explained above. Unfortunately this is not the case. Thus, when the criterion of obedience is satisfied it is impossible for us to apply the criteria of remorse, feelings of guilt, and repentance; and, on the other hand, when the latter criteria are applicable the criterion of obedience is at least partially unsatisfied. It follows that the threatened ambiguity of the phrase 'he thinks he ought . . .' cannot be overcome in this fashion.

Suppose, then, that we say it is the total patterns of results that emerge from the application of as many criteria as possible that determine our judgments of assent to a moral rule. Such an interpretation is suggested by the fact that we do appear to collate the evidence for remorse, feelings of guilt, temptation and repentance. Furthermore, the partial logical independence of our criteria does not seem so fatal to this view as it was to the first that I considered. For we might claim that although our patterns of results are not always the products of applying the same criteria they have a certain common membership, *e.g.* the criteria of obedience and temptation which are always applicable, and this saves the notion of assent from extreme ambiguity. But this claim is a dubious one. Worse, the vagueness implied by the present interpretation does not seem of the kind that we would attribute to our uses of the expression 'he thinks he ought . . .'. But a more fatal objection is that the theory seems unable to accommodate the fact that it is always possible to make rational comparisons of degree of assent when people claim to accept the same moral rule. For how are such judgments to be rationally formed when A satisfies the criteria *p* and *q* and cannot have the other criteria applied to him, and B fails to satisfy the criteria *p* and *q* but satisfies the criteria *r*, *s* and *t*? The problem in such a case is not like that in which a teacher has to estimate the relative scholastic achievements of A and B who have each surpassed the other in some answers to questions of the same examination paper; it is more like attempting to



estimate the relative merits of A and B when they have sat examinations in different subjects.

The superior status of the criterion of recall may be thought to suggest a way out of this difficulty. May we not hold that when the criterion of obedience is fully satisfied we are, as it were, exempted from the application of the other criteria? Such a view seems reasonable since it is precisely the fact that the criterion of obedience has been fully satisfied which prevents the other criteria from being applicable. Its acceptance also equips us with a priority rule for dealing with awkward comparisons; for it enables us to say that the satisfaction of the criterion of conformity is to count for more than the satisfaction of all the other criteria and the partial satisfaction of the criterion of conformity. Awkward cases can still be imagined, of course; but the problems which they present seem to be analogous to those which are successfully handled by teachers when marking examinations. Unfortunately, not all our judgments of relative completeness of assent are consistent with the operation of so simple a priority rule. Thus, it sometimes happens that we attribute a higher degree of assent to a person after he has violated a moral rule than we did before. Suppose that A, who has always acted honestly, loses his money and commits a theft, afterwards suffering from intense remorse. Might we not estimate the degree of his acceptance of the moral rule proscribing theft more highly than we did before, especially if, on independent grounds, we had doubted the worth of his character? If we might, surely we sometimes attach more weight to remorse than we do to conformity? "No", it might be objected, "all we do is to hold our previous estimate of A's character more confidently than we did before." But whether this is so or not, cases of the kind that I am envisaging show that our judgments do not conform with the simple priority rule in question; for clearly, if they did, any failure to conform with a moral rule would be bound to lower our estimate of the agent's degree of assent—and that is not always the case.

None of these views, therefore, can be regarded as satisfactory. What is required is a quite different approach to the problem.

### III

Before we can speak of assenting in some not quite rigorous sense it seems necessary to be clear about what we mean by a full or unreserved acceptance of a moral rule. That is the question to which I shall now proceed.

It seems to be generally admitted that A cannot be said to accept a moral rule unreservedly if he sometimes transgresses it—unless, of course, his violation of the rule is due to its having come into conflict with another moral rule which he also accepts.

Conformity is therefore part of what we mean when we say that someone unreservedly assents to a moral rule. But, as I have already shown, this is not all that we mean; for sometimes we conform with the moral rule for unacceptable reasons. Mere conformity, then, is not enough; the conformity must be attributable to the operation of the right motive.

Suppose that we wish to determine whether A assents to the moral rule, "One ought to return what one borrows". We examine A's conduct and find that he always conforms with the rule. It does not follow that he accepts it, for his reason for obeying it may be a desire to establish a reputation for honesty that will enable him to perpetrate a greater fraud than would otherwise be possible. When, then, should we be satisfied that he does accept it? The answer one feels inclined to make is: when we are satisfied that he conforms with the moral rule because he accepts it. But clearly, we cannot say that by fully assenting to the moral rule, "One ought to do x", one means (i) that the person in question does x, and (ii) that he does x because he assents to the rule enjoining it; for the criterion governing the use of a word can never legitimately contain the word whose use it governs. Although this formulation is unacceptable, however, we may very well feel that it is a defective expression of the answer that we wish to give.

Consider the conduct of A who does x in accordance with the moral rule, "One ought to do x". If A does x because it pays he is making it a rule to do what pays him, and therefore his allegiance is to the rule, "Do what pays you"; if, on the other hand, he does x because he has made it a rule to do x, then he is doing x because he accepts the moral rule, "One ought to do x", and his allegiance is to this moral rule. In the former case his conformity with the moral rule is conditional. If x pays he does x; if x does not pay he does something else which either pays or is expected to pay. The link between the two rules is empirical, not logical; the moral rule is not subsumed under the rule of self-interest; it is simply regarded as a more or less reliable means to conformity with it. We do not require the moral rule to explain what A does; what we require is the rule, "Do what pays you". If, on the other hand, A does x because he has made it a rule, we do indispensably require the moral rule to explain what A does. When this is the case, and A

conforms with the moral rule, the conformity can be spoken of as unconditional. We may say, then, that A accepts the moral rule enjoining *x* without reserve if he makes it a rule to do *x* and always does *x* in the appropriate circumstances unless the moral rule is overridden by another moral rule with which it comes into conflict.

But this statement of the criterion, while it avoids the difficulty which the previous formulation ran foul of, may not seem in harmony with what was said earlier about the necessity of having the right motive. I shall therefore conclude this section with a few remarks about our use of the word 'motive'.

Suppose A is asked, "Why do you do *x*?" He replies, "Because it pays me". Here we have his motive: self-interest. We also have a reference to an explanatory maxim or rule of conduct, namely, "Do what pays you". At least part of the point of A's answer is that it puts us in possession of a predictively reliable rule. If A does *x* because it pays him, the moral rule, "One ought to do *x*", is a less reliable guide to his future conduct than the rule, "Do what pays you". We imply this by saying that doing *x* is a means to doing what pays. When we say that one is a means to the other we have sketched in the conditions under which the moral rule will be observed. When a rule is not observed simply on certain conditions, when there is no more predictively reliable rule to which reference can be made, it is pointless to ask a man for his motive. Thus, if A is asked, "Why do you do what pays you?" all he can say is, "Well, I'm just like that", or "I just do", or "I make it a rule to do so"—unless, of course, he offers us an unhelpful dispositional translation, *e.g.* "Because I'm selfish". Similarly, if B is asked, "Why do you do *x*?" he may reply, "Because it's right". This formula has the same force as, "I make it a rule to do so"—though that by no means exhausts its significance. It points out that conformity is unconditional, and therefore, that it is pointless to look for a further motive.

It must be admitted, however, that whereas we are satisfied with the answer, "Because it pays", and do not press for more fundamental motives, we tend not to be satisfied with the answer, "Because I make it a rule". There seem to be two reasons for this. In the first place, when we are using the language of motives the most satisfactory answer to reach in the end is, "Because I'm like that". It satisfies us because it suggests that we are dealing with a rule of conduct which has the law-like character of being descriptive. It is not regarded as exclusively descriptive, however, for we should want to

maintain that the individual concerned may properly be held responsible for acting conformably with the rule. Thus, although we should deny that he has made it a rule we should insist that he is not compelled to conform with it. It is therefore negatively prescriptive. We prefer law-like to clearly prescriptive rules, provided they are fundamental, because they are thought to govern the conduct of a larger number of human beings, and hence, to possess greater explanatory value. But this preference is sometimes irrational. If A always observes a rule which is not a means to conformity with some other rule this rule is no less indispensably required to understand A's conduct for its being true that it is prescriptive or that it is not required to explain the conduct of the majority of human beings.

In the second place, while there is clearly a limit to the number of questions which one can answer by referring to more and more fundamental law-like rules, it is much less clear that there is a limit to the number of questions one can answer by reference to prescriptive rules. The ground of this difference is that there is a tendency to think of moral and other prescriptive rules as forming systems in a way in which psychological laws do not. Sometimes these systems are conceived of as roughly analogous to logical calculi, of which it can be said that although they must contain axioms and postulates which cannot be questioned, substantially the same calculi can be developed on the basis of different sets of axioms and postulates; hence, they open up the possibility of endless manipulation and reorganisation—endless questioning. At other times these systems are thought of as possessing the kind of coherence which allows us to proceed indefinitely, by questions and answers, in a virtuous circle of mutually reinforcing prescriptions. Now, I think it is sound to attribute some kind of coherence to moral and other prescriptive rules which does not belong to laws of nature (including psychological generalisations, different as these are in some respects from the laws which a physicist attempts to formulate); and hence, I consider it quite proper to ask, "Why do you make it a rule to do  $x$ ?" and thereby to explore the interconnexions of a moral agent's decisions. But once A has accepted a moral rule he can be said to act as he does because he has accepted that moral rule; for, although his decision to conform with it may not have been made if he had not previously arrived at certain other moral decisions, he now regards it as an independent source of obligations. This is to say that our reasons for assenting to a moral rule are related to our acceptance of it

more nearly in the way in which the reasons for a piece of legislation are related to its enactment than in the way our reasons for catching a train are related to our decision to catch it. If I decide to catch a train I may refer to this decision rather than to its grounds if asked why I am unable to meet a friend at a certain time, my reasons being either that this is the simpler explanation or that I am unwilling to review the connexions between my decision and its grounds. Such a decision remains wholly dependent on its grounds, however; for if these grounds were to shift the decision would be automatically affected. Thus, if I am catching a train to London to meet a friend this decision is automatically annulled by his unexpected arrival at the door of my house. The case of an Act of Parliament is very different. The Act can be justified in the sense that reasons can always be given for enforcing actions of the sorts which it prescribes, nevertheless, once it has been enacted it is true to say not only that the actions which it prescribes are enforced because it prescribes them but that they will continue to be enforced for as long as the Act remains an Act even if its effects are not those which were anticipated so that it can no longer be justified in the original way. Thus, in becoming a source of legal obligations the Act has acquired a certain independence of the reasons for its enactment. Moral rules, once they have been accepted, are recognised sources of obligations, and have a similar independence in that the reversal of the moral decisions most closely connected with them does not entail their automatic cancellation. When a moral rule is accepted, therefore, there is a sense in which one cannot get beyond it. In such cases the only motive which can be given for actions conforming with it is the existence of the rule itself. This is what is meant by having the right motive.

#### IV

When I say that A accepts the moral rule, "One ought to do x", without reserve, I mean (i) that he always obeys this moral rule, and (ii) that his obedience is unconditional in the sense that it is to be explained by reference to the rule. In my view, the weaker criteria are derived from this strict criterion by a process of stretching or of dilution of meaning. These criteria shade into one another forming a descending scale of strictness. There are only two additional formulations, however. These are: (1) A accepts a moral rule if (a) he intends

to obey it, and (b) his intention to obey it is unconditional (2) A accepts a moral rule if (a) he wishes to obey it, and (b) his wish to obey it is unconditional.

There are three preliminary points which I wish to make with regard to these formulations. First, it is easy to see that they are derived from the criterion of full assent by means of a process of stretching, leading to the thinning out of meaning. Obedience includes both the intention and the wish to obey; and the intention to obey includes the wish to obey. Secondly, these words, 'obey', 'intention to obey', and 'wish to obey' operate in such a manner as to give rise to a continuous scale of weakening criteria such as is implied by such terms as 'stretching' and 'elasticity'. Thirdly, there is something unconditional about each of these formulations; for without some kind of unconditionality one cannot speak of assenting in any degree whatsoever, since without unconditionality of obedience, or intention, or wish, no reference need be made to the relevant moral rule to explain how the agent behaves, in which case it becomes meaningless to speak of his having made it a rule to act in a certain way.

(1) is the formulation of that part of the scale of criteria we most commonly employ. It is suitable for application to people who normally, but not invariably, measure up to their standards. If they did not usually conform with the moral rules they claim to accept we should not be prepared to speak of them as intending to conform with them. Their lapses can be held not to involve any change of intention if they are unpremeditated and are followed by signs of remorse, feelings of guilt and repentance.

(2) formulates that part of the scale of criteria which we use to speak of the incontinent, as Aristotle calls them, e.g. of alcoholics. An alcoholic may seldom resist the temptation to drink to excess yet we may be prepared to grant that he assents to the moral rule, "One ought to be temperate in one's consumption of alcohol", if he invariably struggles before succumbing to temptation and feels remorse, etc., when he is sufficiently recovered from his lapses to know that it was he who hit the bottle and not the bottle which hit him. These signs of remorse, struggle, etc., are interpreted as proceeding from an unconditional wish to obey the moral rule which he so frequently transgresses.

The more exacting the criterion which a moral agent can satisfy the higher the degree of assent we attribute to him.

## V

I shall now briefly consider whether the view that I have outlined is able to cope with the difficulties that emerged in Section II. These difficulties point to five requirements of an adequate theory, namely: (i) that it accounts for the vagueness of 'he thinks he ought . . .' without implying that that phrase is grossly ambiguous; (ii) that the relations which it holds to obtain between the criteria should involve no logical difficulties; (iii) that it should account for the special importance that we attach to conformity without implying an untenably simple priority rule; (iv) that it should be consistent with rational comparisons of degree of assent; and (v) that it should be able to explain certain anomalous judgments of relative fullness of assent. As far as I can see at present these five requirements are satisfied by the above theory.

Although I assert a multiplicity of criteria my view does not imply that 'he thinks he ought . . .' is ambiguous; first, because the criteria form a scale and are derived from a single strict criterion, and secondly, because there are no discontinuities in the scale which they form. The proper word to apply to a phrase whose use is governed by such a scale of weakening criteria is not 'ambiguous' but 'vague'; and it has already been admitted that 'he thinks he ought . . .' is a vague phrase.

It can also be claimed that the theory does not give rise to logical difficulties of the kind which proved fatal to several of the views which I examined in Section II.

Again, the theory seems consistent with rational comparisons of degree of assent. These can be made by comparing the relative rigorousness of the criteria which are satisfied by different individuals or by the same individual at different times. One cannot be said to assent unless one satisfies some criterion; and all the criteria which one might satisfy belong to a single scale. It follows that it is always possible to compare the criteria which different individuals satisfy.

It seems equally successful in accounting for the special importance of conformity without implying a simple priority rule. Conformity is an essential part of the criterion of full assent. Remorse, on the other hand, is only evidence for either the wish or the intention to conform, both of which are clearly less satisfactory than conformity itself. But one can conform from the wrong motives. Hence there are times when we attach more weight to remorse than we do to conformity in our judgments of relative fullness of assent; that is to say, those



judgments in which we appear to up-grade individuals after they have failed to conform with a moral rule they claim to accept. These judgments may be interpreted in two ways : (i) as the attribution of a higher degree of assent to individuals ; and (ii) as an accession of confidence with regard to the rightness of previous estimates of their degree of assent. The theory has room for both interpretations. Suppose A, once rich and seemingly honest, loses his money and resorts to fraud on a single occasion. If he feels remorseful and makes reparation (as is required by our criteria of repentance), we may up-grade him. At first sight this is hard to explain for his failure to conform necessitates his being judged by a lower criterion than might previously have been applied to him. But on grounds of his general character as this was revealed to us outside the sphere of business relationships, together with the absence of temptation to transgress the rules of honesty, we may have previously supposed, in spite of his conformity, that these moral rules meant little or nothing to him. Now we do repose some confidence in his claims to accept them. He therefore rises in our estimation in spite of his moral lapse. The theory accommodates the second interpretation still more easily ; for it is obvious that there is often little evidence to go upon till after a man has transgressed the moral rules to which he claims to assent.

## VI

By way of conclusion I shall now attempt to relate what I have said to the question of weakness of the will.

It seems that we place men high or low upon the scale of strength of will or character according as we estimate their actions, intentions and wishes to be relatively united or disunited. But the nature of this scale is not immediately clear. For while we may feel some hesitation in saying that a weak man does what he intends we feel no hesitation in saying that, in some sense, he does what he wishes. Of the three words 'action', 'intention' and 'wish' it is therefore the word 'intention' that tends to disappear when we are talking about extreme weakness of will. This may be attributed to the fact that 'intention' suggests premeditated behaviour, and part of what we want to say is that the extremely weak-willed are largely at the mercy of their impulses, and hence, that they seldom pursue a settled plan of action.

But this can easily be exaggerated. Even alcoholics are sometimes capable of premeditated behaviour; indeed, they tend to follow settled plans of action because of the unbalanced nature of their desires. Hence, it is not the difference between premeditated and unpremeditated behaviour which most needs to be stressed; a more important distinction is that between behaviour which conforms with law-like rules and behaviour which conforms with clearly prescriptive rules. Alcoholics and other weak-willed people are often painfully predictable; but the maxims of their conduct (in Kant's sense) have power in their lives without possessing authority; and the rules which they attempt to impose on themselves have authority without possessing power. And herein lies their weakness—their failure to translate this authority into power. In people of strong character these rules hold dominion so that their actions are relatively predictable like those of the extremely weak-willed; the difference is that whereas the weak-willed man's actions only conform with law-like rules the strong-willed man's actions conform with clearly prescriptive rules, moral or otherwise. The moral rules which a strong-willed man accepts invade his life and totally subdue it; the moral rules which a less strong-willed man accepts only control his intentions so that some of his actions escape their dictation; and finally, the moral rules which a weak-willed man accepts do not permeate his life beyond the level of idle wishing so that its course proceeds almost entirely outside the pale of the rules to which he assents. It is the actions, intentions, and wishes, then, which have a bearing upon the dominion of prescriptive rules that we examine when estimating weakness of will. In the strongest men these are, as it were, united; in the weakest men they fall utterly apart.

The scales of fullness of assent and strength of character are therefore closely connected. Those who fully assent to moral rules are persons of strong character; those who only partially assent to moral rules are persons of weak character. But the scales are nevertheless distinct, the latter being used for a more comprehensive set of judgments. Thus, it does not follow from a person's having either a strong or a weak character that he accepts any moral rules, although it does follow from his having a strong character that he fully accepts some prescriptive rules, e.g. those of self-interest, and from his having a weak character that he does not fully assent to any prescriptive rules.

But is this a satisfactory view of the relations of the two scales? Are there not occasions when we wish to say that a

strong man accepts a moral rule with a low degree of assent ? Although at first sight there do seem to be such occasions, I think they are mainly to be explained by the ambiguity of the phrase, 'a strong man', which sometimes means 'a person of strong character' and sometimes 'a man whom other men readily fear or obey'. It seems to me that when men of strong character claim to accept a moral rule with which they seldom conform we normally maintain that they are lying, hoping to deceive us for some purpose of their own. Occasionally, however, we seem to accept their claims. But when we do so we no longer regard them as men of strong character. Instead of interpreting their conduct as the expression of a strong will we now view it as obsessive in character, arising out of uncontrollable urges which, unlike those of the alcoholic, can be mistaken for the operation of fully accepted rules of a non-moral nature. Thus, we rapidly turn from one idiom to another, the adjustment being necessitated by our acceptance of the claims that have been made. I think, therefore, that the modes of speech that we employ in connexion with the men whom we call strong, far from being a source of difficulty, actually confirm the rightness of the view that I have adopted with regard to the relations of the scales of fullness of assent and strength of will.

*Edinburgh University*

## V.—INDEXICAL EXPRESSIONS

BY YEHOShUA BAR-HILLEL

### I

EVEN very superficial investigation into the linguistic habits of users of ordinary language will reveal that there are strong variations in the degree of dependence of the reference of linguistic expressions on the pragmatic context of their production. Whereas, for instance, the sentence

(1) Ice floats on water

will be understood by almost every grown-up normal English-speaking person to refer to the same state of affairs (this statement needs, strictly speaking, some qualifications which, however, in view of their generality, do not disturb the distinctions we are going to make), what the sentence

(2) It's raining

is intended to refer to will be fully grasped only by those people who know the place and the time of its production, and the identification of the intended reference of the sentence

(3) I am hungry

will require the knowledge of its producer and the time of its production.

I hope that the reader has noticed various ambiguities in the first paragraph and I hasten to straighten them out. I shall use in this paper the term 'sentence' in its traditional grammatical connotation, so that (2), for instance, will be regarded as a full-grown sentence rather than a mere abbreviation of a sentence, as modern logicians like to have it. And, secondly, I have, of course, to introduce immediately the now well-known type-token terminology with respect to the various usages of 'sentence'. Using this terminology, we may say that all the tokens of the sentence-type (1) will be understood by almost every grown-up English-speaking person to refer to the same state of affairs, whereas nothing of this kind can be said with respect to the tokens of (2) and (3).

Assuming this to be the case, we are entitled, according to a common and extremely important procedure, to abstract from the pragmatic context of the production of the various tokens of (1) altogether and say that all the tokens of (1) have the same reference. Most people and many philosophers would even

speak of the common reference of the sentence-type (1). I shall adhere—as I did already in the first paragraph—to this usage without any ontological commitments, *i.e.*, I am ready to regard it merely as a form of speech. In any case, I am for the moment completely uninterested in the ontological status of the references of sentences.

What is, however, important, for our purposes, is that not all the tokens of (2) have the same reference, though some of them may have, and that we are therefore not entitled to speak of the reference of (2) even as a form of speech. With respect to (3), we might even safely say that no two of its tokens have the same reference, since even if they are produced by the same person, say A, their production takes place at different times, say  $t_1$  and  $t_2$ , so that one token will have the same reference as (any token of) 'A is hungry at  $t_1$ ' (where 'is' is to be understood tenselessly) and the other token will have the same reference as 'A is hungry at  $t_2$ '.

It follows that the abstraction from the pragmatic context, which is precisely the step taken from descriptive pragmatics to descriptive semantics, is legitimate only when the pragmatic context is (more or less) irrelevant and defensible as a tentative step only when this context can be assumed to be irrelevant. It is, therefore, just a mistake to deal with references of the sentence-types (2), (3) and their like. Though denying a reference to type (2), we might still say it has a *meaning* in this sense that its various tokens may fulfil the same pragmatic function (or the same pragmatic functions), say to draw attention to certain meteorological conditions in the space-time neighbourhoods of their producers (or in certain other specifiable neighbourhoods). It seems, however, advisable to avoid the overburdened term 'meaning' in this connexion, and I propose, therefore, to use instead the term '*function*' (as short for 'pragmatic function').

Similar things hold with respect to truth. Every token of (1) is true, and since all the tokens of (1) have the same reference, we may say in short that (1) is true. That the 'since'-clause in the former sentence is necessary is shown by

(4) I am producing now a sentence-token,

every token of which is true but which, nevertheless, cannot be regarded as true itself, since it does not refer to anything. (This "paradoxical" situation and a similar one arising with respect to the next example (5) will be discussed somewhat fuller in section VIII.)

Not all (possible) tokens of (2) or (3) have the same truth-values; with respect to these types, it is even more obvious that it makes no sense to speak about their truth or falsity.

To simplify the terminology, I shall call a sentence-token which is either true or false a *statement*-token and its reference a *proposition*. If and only if each sentence-token of the same type has one and the same proposition as its reference, I shall call this type a *statement(-type)*. According to this usage, only (1) will be a statement, but neither (2) nor (3) nor (4), though all tokens of these sentences are statements and all tokens of (4) even true statements.

This is a disquieting situation, though rather obvious and therefore often noticed. I have, nevertheless, the impression that its implications have not always been understood and its consequences not often been drawn and certainly not to a sufficient degree.

But before I proceed to draw what seems to me to be the necessary consequences of our insight, I should like to point out that the account given so far is still oversimplified to such a degree that obviously relevant factors have been left out. Is it, for instance, really the case that *all* tokens of type (3) are statements? Would we like to regard as a statement the utterance of such a token by an actor on the stage? Or the writing of another token of (3) in the sand of the desert by some strange play of the winds? I certainly would not, at least when the pragmatic context of the production of these tokens were known to me. For a sentence to be a statement, it has to fulfil certain syntactic and semantic conditions, but we see now that, in addition, its production has to fulfil certain pragmatic conditions, too, such as being produced by a conscious being having a certain "propositional attitude"; we shall not enter a discussion of the specific required conditions. We learn also from these situations that sometimes a sentence-token which is not meant by its producer to be a statement is understood so by a listener or reader, and we can, of course, very well imagine the opposite situation.

The extraordinary status of (4) is a bit shaken by these considerations, because now it turns out that not all imaginable tokens of this type are true, as we assumed before somewhat rashly. The situation is similar with respect to another "paradoxical" sentence-type

(5) I am dead.

It is, once again, not exactly the case that all tokens of this type are false, as has been assumed by some authors, though we

might still say that all those tokens which are capable of being either true or false, the statements among them, cannot fail to be false.

One is, of course, entitled to introduce the additional pragmatic conditions into the definition of 'sentence' itself and to deny this predicate to patterns which "look like sentences" but the production of which does not fulfil the mentioned conditions. Such patterns might then be called perhaps "sentence-like patterns". But I still would not agree to Gilbert Ryle's formulation<sup>1</sup> that an actor's utterance cannot be classified as either "use" or "mention". It is true that an actor does, in one important sense, neither use statements nor state propositions, but he still uses sentences and perhaps even mentions propositions. In another sense, however, he even uses ("fictitious") statements and states ("fictitious") propositions.

These considerations make no difference with regard to sentence-type (3). It is not a statement, anyhow. But what with regard to (1)? We decided before to call it a statement, assuming that *all* its tokens have practically the same reference. But now we see that this is not the case—there might be some tokens of this type which have no reference at all, are not at all statements. Well, I still believe that it is advantageous to stick to our former decision. The cases where a token of (1) is not meant to be a statement are surely rather exceptional and anyhow completely harmless, since in these exceptional cases the tokens of (1) do not have a reference different from that possessed by the regularly produced tokens but no reference at all, and to abstract from them, therefore, is still in line with common scientific procedure. But let us not forget these exceptions, else we shall find ourselves sometimes confronting self-created pseudo-problems.

Another preliminary clarification has to be given to the term 'sentence-token'. I shall use it in such a way that a sentence-inscription, for instance, will be regarded as "the same token" during its whole life-time. More technically speaking, sameness of linguistic tokens will be defined by *genidentity*. Accordingly, we may say that many people read the same sentence-token, and this even at different times.

This procedure is by no means necessary, and it has rather awkward consequences, in certain cases. But other procedures will have their disadvantages, too, and it seems to me that for our investigation the decision chosen is the least evil.

<sup>1</sup> In "If", "So", and "Because", *Philosophical Analysis*, 1950, pp. 323-340.



## II

Let us turn now to our main theme. Having grasped clearly that it is meaningless to speak about the truth, or even reference, of the sentence-type (3), a conviction we summarized in denying this type, and similar ones, the title 'statement', we may now ask about the reference and truth of a certain *token* of this type. To what, then, does a certain token of 'I am hungry' refer? To the fact that I am hungry? Is this token true if and only if I am hungry? Certainly not! Outrageous as this interpretation sounds, let us notice, in passing, that the truth of sentences of this kind does not fulfil the famous Leśniewski-Tarski criterion of adequateness, at least not in its unsophisticated version which is meant to hold for statements of type (1). We may even turn the tables and safely declare that the inapplicability of this truth-criterion to a given sentence-type is a criterion that this type is not a statement.

What then, to ask the question again, does a certain token of (3) refer to? I hope that the reader is now ready to see that no categorical answer can be given to this question, so long as the pragmatic context of the production of this token is not known. Only when we know that it had been produced by B at time  $t_3$ , when he was fully conscious, not reciting a part of a play, etc., can we say that it refers to the same proposition as any token of 'B is hungry at  $t_3$ ', a type of statements for which we assume that all questions of reference and truth are settled, so that this answer will satisfy us. Not knowing the pragmatic context, we can answer only hypothetically, with the help of a subjunctive conditional, "If this token had been produced by C at  $t_4$ , it would have meant that C is hungry at  $t_4$ ", or using a general conditional, "For every person X and every time t, if X produces a token of the type 'I am hungry' at t (in an appropriate mood) this token refers to the proposition that X is hungry at t". And such a token of 'I am hungry' produced by X at t will be true if and only if X is hungry at t.

But I am not yet satisfied. I am afraid that certain modes of expression which I have used so far and which are in full accord with the common ways of argumentation are dangerously misleading and have, in fact, misled many philosophers. I refer to the expression 'a token of (3) has a definite reference in a certain pragmatic context', which may be paraphrased as 'a token of (3), as produced by C at  $t_4$ , refers to the proposition that C is hungry at  $t_4$ '. The adverbial clause 'in a certain pragmatic context', or the sentential clause 'as produced by C

at  $t_4$  are logical danger signals. They give the impression of being not too essential qualifications which cannot change the intrinsically dyadic relation of reference holding between a sentence-token and the proposition expressed by it. But this would be a mistake. Since the pragmatic context is essential and its omission leaves the token without reference, we have before us an essentially triadic relation between token, context, and proposition. We are entitled, of course, to analyse, for certain purposes, the context further into producer, recipient, the time of production, the place of production, etc., and get thereby polyadic relations with 4, 5, or more terms. And we are, of course, entitled, if we wish so for certain purposes, to reduce the triadic relation to a dyadic one, but—and this is the essential point—in this case the one member of the relation would not be any more the token itself, but the ordered pair consisting of the token and the context. Shifting the context to the other side of the relation, though formally completely correct, would be less in agreement with our usual linguistic habits and would anyhow change nothing at all.

There is nothing new in my point. It has been stressed often enough that it is not a sentence-token that refers to a proposition, but that it is a person who refers to something by this token (or a person-like machine, to keep up with the latest developments of communication-theory). And this is certainly true. But it is, on the other hand, also admissible and fruitful to speak about the reference of a sentence-token, if the context is irrelevant. And I am not sure whether this vital distinction, though nothing more than a restatement of the conditions which allow the transition from descriptive pragmatics to descriptive semantics, has always been fully understood.

The whole situation deserves more careful study with the help of an appropriate symbolism. Let me hint here at one possible start. Taking the triadic relation “(the sentence)  $a$  refers-pragmatically-to (the proposition)  $b$  in (the pragmatical context which includes also a reference to a language)  $c$ ” as an undefined primitive concept—in symbols:  $RP(a, b, c)$ —one could define “ $a$  refers-pragmatically-in- $c$ -to  $b$ ”—in symbols:  $RP_c(a, b)$ —and “ $a$ -in- $c$  refers-pragmatically-to  $b$ ”—in symbols:  $RP^+(a; c, b)$ —as being synonymous with it and then define ‘refers-semantically’ on the basis of one of these concepts, e.g.  $RS(a, b) = df (c) (d) (RP(a, b, c) \equiv RP(a, b, d))$ .

I propose not to assign reference and truth to sentence-tokens of type (2), (3), and their like, but only to a sentence-token-in-a-certain-context, i.e. to the ordered pair consisting of the

sentence-token and its context. For the purpose of shorter expression, I shall call such a pair a *judgment*, thereby distilling a new sense out of this old-fashioned term. I hope the reader will carry in mind that, in this article, a judgment is neither the judged sentence-token nor the process of producing this token, but nothing more and nothing less than just the pair consisting of the token and the context.

By this proposal, I have reversed my former decision to call all tokens of (2) to (5), inclusive, statements. To leave no doubt open and to round off my terminology, I shall now make my final terminological proposals:

I shall use '*sentence*' (with respect to ordinary languages) as it is customarily used by grammarians.

Ordered pairs of sentences and contexts, of which truth or falsity may be predicated, will be called *judgments*.

The first component of a judgment will be called a *declarative sentence*.

A declarative sentence which paired with any context whatsoever forms judgments which refer always to the same proposition will be called a *statement*, otherwise an *indexical declarative sentence*, shortened to *indexical sentence* whenever misunderstandings will not be likely to arise.

The terms 'proposition', 'judgment', 'declarative sentence', 'statement', and 'indexical sentence' can be easily defined on the basis of the mentioned primitive triadic relation, its derivatives, and 'sentence'. One example will do for the purpose of illustration:

$$a \text{ is a statement} =_{df} (Eb)(RS(a, b)).$$

The distinction between 'indexical' and 'non-indexical' could, of course, be drawn also with respect to other types of sentences. "Close the door!" would be an indexical, "A to close the door  $d_1$  at  $t_1$ !" a non-indexical command. But I shall limit my discussion to declarative sentences only.

All the definitions given refer, of course, to tokens only. The definitions of 'sentence-type' and 'statement-type' are obvious. And it clearly makes no sense to speak about 'judgment-types'.

According to these definitions, all the tokens of (1) and none of the tokens of (2) to (5), inclusive, will be statements, but the pairs of all of them with their pragmatic contexts will be judgments.

With respect to most artificial interpreted language-systems, our terms 'sentence', 'declarative sentence', and 'statement' coincide, whereas 'indexical sentence' is empty and 'judgment'

unimportant. These systems have been constructed so that the pragmatic contexts of the production of their sentences is completely irrelevant. I believe that Carnap was the only major logician who mentioned this point explicitly, when he decided<sup>1</sup> to restrict his discussion of General Syntax to languages featuring this property only. He also drew the important distinction between two types of context-dependency (Carnap's own term is 'extra-syntactical dependence' which might be somewhat misleading outside the framework of Logical Syntax): an inessential one where the relevant context consists of preceding sentences only exemplified by "Yes" as an answer to "Does ice float on water?", where "Yes" is immediately replaceable by the statement (1); and an essential one, where the relevant context is extra-linguistic, which is much more interesting and therefore the main one discussed in this paper.

Owing to the restriction to non-indexical languages—voluntary and explicit with Carnap, unconscious with most other logicians—the tremendous development of Logical Syntax and Semantics in the last two decades has had only limited bearings on indexical languages, and no satisfactory *logic of judgments* has been proposed so far, although judgments with indexical components play an extremely important role both in common and in philosophical discourse. I have no statistics available, but I guess that more than 90 per cent. of the declarative sentence-tokens we produce during our life-time are indexical sentences and not statements; it is plain that most sentences with tensed verbs are indexical, not to mention all those sentences which contain expressions like 'I', 'you', 'here', 'there', 'now', 'yesterday' and 'this'.

What can be the explanation of this strange neglect of such very obvious traits of ordinary languages? I venture the following hypothesis: Since a judgment with an indexical sentence as first component can always, without loss of information, be transformed into a judgment with a statement as a first component, keeping the second component intact, we might easily be tempted to drop the common phrase 'a judgment with . . . as first component' from both sides of this transformability statement and arrive at the result that any indexical sentence can be transformed into a statement, a patent falsity, according to our former analysis. I guess that this illicit dropping is the main cause for the mentioned neglect, which is then in its turn the main cause for the grave mistakes

<sup>1</sup> In *The Logical Syntax of Language*, London, 1937, § 46, p. 168.

made sometimes by good philosophers and logicians in the use and mention of indexical sentences.

Assuming that our psychological explanation holds for those cases in which treatment of the indexical traits of ordinary languages has been unconsciously neglected, we still have to face the deliberate neglect practised, for instance, by Carnap. I believe that the two main reasons behind his decision were: First, non-indexical languages are sufficient for the formulation of any given body of knowledge; second, the logic of non-indexical languages is complicated enough and should be developed before we proceed to deal with the incomparably more complicated logic of indexical languages. He was right in both reasons, in their time. But now, since the development of a satisfactory logic of non-indexical languages is well under way and since formulation of given bodies of knowledge is obviously not the only function of language, we cannot shun any more the more formidable task of analysing the complicated functioning of indexical expressions.

But is not the formulation I gave to the first reason weaker than necessary? Could we not assert much more, namely that non-indexical languages are sufficient for *every* communicative purpose? If this were true, if one could always express every cognitive content in a non-indexical language, the urgency of an investigation of the logic of indexical languages would be somewhat reduced, though it would still be of extreme importance for the analysis of common and philosophical discourse as it is historically given.

Let us try to answer this question with the help of the following *Gedankenexperiment*. Assume that Tom Brown is a logician interested in our problem who has decided to find out whether he could get along, for just one day, the first of January 1951, using the non-indexical part of ordinary English only. He told, of course, his wife about this experiment. At the morning of the mentioned day Tom awakes and since it is a holiday, he decides to have breakfast in his bed. His watch is under repair and he, therefore, does not know the time. How shall he inform his wife about his wish? He is forbidden to say 'I am hungry', but even 'Tom Brown (is) hungry on January 1st, 1951' will not do, since nothing in this sentence (though, of course, many things outside the sentence) indicates that he is hungry then, rather than that he has been hungry before or will be hungry in the afternoon. And he has told his wife to react only to the sentences themselves and to nothing else. Shall he say, then,

Tom Brown is hungry at the moment when Tom Brown utters this sentence-token ?

Certainly not. He is not allowed to use 'this'. Well, then perhaps

Tom Brown is hungry at the moment when Tom Brown utters 'Tom Brown is hungry',

or, say,

Tom Brown is hungry at the moment when Tom Brown utters 'tweedledum',

or, even,

Tom Brown is hungry at the moment when a fly is sitting on his bed

will do ? No, still not. There is nothing in the 'when'-clauses which ensures the uniqueness of the described situation, and Tom Brown, as a good logician, could not even use the definite article 'the' before 'moment'. Does there exist another uniqueness-ensuring 'when'-clause ? Perhaps. I must admit that if I had been in his place, I would not have managed to make myself understood to my wife (or should I say 'to his wife' ?) to the same degree as a simple 'I am hungry' would have done under ordinary circumstances. (This point should not be exaggerated. For successful communication it is not necessary that the event-class described in the 'when'-clause should be logically unique. A plausible empirical uniqueness will do in most cases. And it is a grave mistake to suppose that indexical expressions function always better in this respect. It is well known that many failures of communication are due to an excessive use of 'this'.)

But even if Tom Brown had a good watch and were able to say

Tom Brown is hungry on January 1st, 1951, at nine o'clock in the morning,

but his wife had none and no other means to check the time, he would have failed to communicate what he wanted to.

We see that effective communication by means of indexical sentences requires that the recipient should know the pragmatic context of the production of the indexical sentence-tokens. (Sometimes other contexts are relevant—but I shall waive their discussion.) To communicate the same amount of information by using non-indexical sentences only, knowledge of the context by the recipient is not required, but in its stead

additional knowledge of some other kind may be necessary. Not in every actual communicative situation could every indexical sentence be replaced, without loss of information, by a non-indexical sentence; but there is, on the other hand, no indexical sentence which could not be replaced by a non-indexical sentence, without loss of information, in some suitable communicative situation.

Since our knowledge is limited, the use of indexical expressions seems therefore to be not only most convenient in very many situations—nobody would doubt this fact—but also indispensable for effective communication. Indexical language will continue to be used by scientists, philosophers, and everybody else alike. Recipients of indexical communication will not always be able to know its original context and hence not be able to find the statement to which the received sentence, paired with its context, is logically equivalent. Interesting and important problems with regard to successful or unsuccessful communication are certain to arise. I believe, therefore, that the investigation of indexical languages and the erection of indexical language-systems are urgent tasks for contemporary logicians. May I add, for the sake of classificatory clarity, that the former task belongs to *descriptive pragmatics* and the latter to *pure pragmatics* (in one of the many senses of this expression)?

### III

Our investigation was so far carried on mainly on the level of sentences. It can be repeated on the level of non-sentential expressions, of course only with respect to reference and not with respect to truth. This has been the customary procedure, so far. It was C. S. Peirce who introduced the terms 'indexical sign' and 'index', Bertrand Russell used instead 'ego-centric particular', Nelson Goodman coined 'indicator', and Hans Reichenbach 'token-reflexive word'. I decided to use Peirce's term since it provides an adjective easily combined with 'sign', 'word', 'expression', 'sentence', 'language', 'communication' alike. I already made use of these combinations before, without any formal definition. Since these definitions are more or less obvious, on the semi-strict level of our exposition, I shall skip them. Let it be stressed, however, that an indexical sentence need not necessarily contain a non-sentential indexical expression as a part: 'Rain', for instance, can serve, in suitable contexts, as an indexical sentence.



## IV

Let me deal, though only in a few lines, with some of the problems in communication arising out of the use of indexical language. One major problem lies in the fact that the pragmatic context, as it is known to the producer of the expression and which is, *nota bene*, not formulated but assumed to be tacitly understood in any act of communication, need not be understood in this way by a recipient and may be understood in various ways by various recipients. The depth of the pragmatic context which is necessary for the full understanding of various sentence-tokens is different, of course, from case to case and cannot even be brought into a linear order. To grasp the reference of a token of (2), for instance, one has to know more in one respect but may also know less in another respect than with regard to (3). To understand a token of (2), we have to know *where* this token has been produced, which is not necessary with regard to a token of (3); but we need not know *who* has produced it, which is necessary in the other case. (I use the expression 'the reference of an indexical sentence' for the reference intended by the producer of this token, although I have doubts as to the usefulness of such an expression. I also use 'understand' in its strong sense of 'fully understand', i.e. 'understand in the way intended by the producer'; in another weaker sense, one can "understand" a token of (2) or (3) if one knows only that it has been produced by somebody in an appropriate mood. (2) will then be understood to mean that it is raining sometime somewhere and (3) as 'Somebody is hungry sometime', which are good statements, though, of course, of extremely poor content; in another weaker (or broader) sense still, *misunderstanding* will also be a kind of understanding, and somebody who, for some reason or for no reason, took M to be the producer of a token of (3), whereas this token was produced by N, will still have "understood" this token.'

Let me also just mention a brand of dependency which embraces even the non-indexical sentences. I mean the fact that any token has to be understood to belong to a certain language. When somebody hears somebody else utter a sound which sounds to him like the English 'nine', he might sometimes have good reasons to believe that this sound does not refer to the number nine, and this in the case that he will have good reasons to assume that this sound belongs to the German language, in which case it refers to the same as the English 'no'. In this

sense, *no* linguistic expression is completely independent of the pragmatic context. But just because this kind of dependence is universal, it is trivial, and we shall forget it for our purposes. (May I, nevertheless, add in parentheses, that all expressions dealt with in our investigation belong to ordinary English, if not specifically mentioned otherwise ?)

Though indexical communication is in so many cases much more convenient than non-indexical and often even indispensable, it carries with it additional possibilities of misunderstanding. Mistaking A for B as the producer of a token of (1) will not cause misunderstanding of this statement. But this will happen with respect to (3). There might arise cases in which both indexical and non-indexical transmission of information or commands will be possible. Weighing the respective advantages and disadvantages in such cases would be an important problem in applied semiotics. Usually, but not always, additional possibilities of misunderstandings will be a price not too high paid for shortness, straightforwardness, and other advantages of indexical communication. The main danger in indexical communication is, however, not in *this* obvious fact but in another one, namely that the dependence upon pragmatic context might sometimes be forgotten so that the recipient will tend to supplement unconsciously some context, but not the intended one, to the received expression-token and so get the impression of having received a statement-token with no special problems of reference.

I have left the central concept of this paper, namely *pragmatic context*, in rather thorough vagueness, and this for the very simple reason that I see no clear way to reduce this vagueness at the moment. From a technical point of view, it seems to me preferable to replace the contexts by *context-descriptions*. (Contexts are non-linguistic events, context-descriptions are linguistic entities.) This procedure would have the advantage that the pairs would be more homogeneous, consisting now of two linguistic components instead of one linguistic and one non-linguistic component. But, as against this, it should be borne in mind that a context-description might be more specific than the context which the producer might have had "in his mind". The vagueness in which I left the expression 'pragmatic context' is partly due to the fact that its reference is often intrinsically vague itself.

Let me also call attention to the fact that of two tokens, belonging to the same type, the one may be indexical and the other not and that sometimes the very same token may be

understood indexically by A and non-indexically by B. A token of 'Socrates has an ugly wife' (with a tenseless 'has') will be understood by most educated people in the year 1951 non-indexically, but there may be a few people who have acquaintances called 'Socrates'—with or without ugly wives—and would therefore like to know the pragmatic context of the production of this token before they are able to make up their mind with respect to the reference and truth of this token. To say that what the grammarians call 'proper names' in ordinary languages are not always (or perhaps never?) "really" or "logically" proper names is just one way—and not the best one—of explaining this somewhat inconvenient fact.

## V

The question of the interdefinability of the various indexical expressions has been discussed quite often in recent years. It is, however, still not customary among philosophers to distinguish such a question in descriptive pragmatics of ordinary languages where 'interdefinability' would be synonymous (more or less) with 'universal replaceability without loss of information' from verbally the same question with respect to a language-system supposed to stand "in close connexion" with ordinary language. In the second case, the question would belong to pure special pragmatics, if the language-system is pragmatical, or to pure special semantics, if the system is semantical. With all the vagueness left by this non-distinction in mind, let us now turn to an investigation of some of the answers given.

Russell<sup>1</sup> believes that all indexical expressions can be defined with the help of non-indexical expressions plus the single indexical word 'this', and he is followed in this respect by Reichenbach,<sup>2</sup> Arthur Pap,<sup>3</sup> and others, sometimes with slight variants. Russell says, for instance, at one place that "I" means "the person experiencing this" and defines in another place "I" as "the person attending to this". It is, however, pretty obvious that Russell's statement, understood in the sense that 'I' can always and without loss of information be replaced by 'the person experiencing this', is false, and this because it is simply not at all the case that "given the speaker and the time, the

<sup>1</sup> In *Human Knowledge*, London, 1948, ch. iv, pp. 100 ff.

<sup>2</sup> In *Elements of Symbolic Logic*, New York, 1947, § 50, pp. 248 ff.

<sup>3</sup> "Are Individual Concepts Necessary?", *Philosophical Studies* I (1950), p. 22.

meaning of 'this' is unambiguous", as everybody will verify immediately: Knowing *only* the speaker and the time of utterance of 'The person experiencing this is hungry', we would not yet be justified in understanding that the speaker was hungry at the time of the utterance of this token (though we would probably fail only seldom if we understood it in this way), whereas we could do so unhesitatingly on hearing 'I am hungry' and knowing once again the speaker and the time of utterance only. It is a very plain fact that whereas the *function* (not, of course, the *reference*) of 'I' is unambiguous, the function of 'this' is decidedly not so. 'This' is used to call attention to something in the centre of the field of vision of its producer, but, of course, also to something in his spatial neighbourhood, even if not in his centre of vision or not in his field of vision at all, or to some thing or some event or some situation, etc., mentioned by himself or by somebody else in utterances preceding his utterance, and in many more ways.

## VI

Sometimes the question of reducibility of the indexical expressions to just one of them has been discussed together with the question of their complete eliminability, a problem we discussed before (in section II). And, indeed, a positive solution of the first would simplify the solution of the second since the eliminability of one indexical expression only had now to be brought under consideration.

When I arrived at the result that indexical communication is indispensable, I was in good company. Peirce,<sup>1</sup> Russell, Arthur W. Burks,<sup>1</sup> and Pap came to the same conclusion. But their arguments seem to me to be wrong and of sufficient interest to be discussed here. If I have understood them rightly, their main argument is that replacing 'here' or 'now' by co-ordinate-descriptions does not eliminate the indexical space and time descriptions since the origin of the co-ordinate-system, to which the replacing co-ordinates refer, the directions and the units of its axis can be taught and learned only with the help of indexical, linguistic or non-linguistic, signs. But this seems to me to be a very obvious *non sequitur*, based on a *confusion between*

<sup>1</sup> Peirce's theory of indexes is given a good exposition and critical discussion by Burks in "Icon, Index, and Symbol", *Philosophy and Phenomenological Research* ix (1949), pp. 679 ff. Burks's own later treatment of the problem shows decisive advances towards better understanding of the specific functioning of indexical expressions.

using language and learning how to use language. There can be little doubt that learning how to use co-ordinates, just as learning how to use words like 'red', involves the use of indexical signs. But, nevertheless, a co-ordinate, just as the word 'red', is non-indexical in this clear and definite sense we used this term in our discussion, namely, to say it again, in its reference being independent of the pragmatic context of its production. A token of 'This book is red' will not be understood in the way intended by its producer by anybody who does not know the context of its production, even if he has an encyclopedic knowledge and an arsenal of tools; a token of 'The book at location  $l_1$  and time  $t_1$  is red' will be understood in exactly the same way by anybody having a certain knowledge (which he might have got with the help of indexical signs) and perhaps other tools.

Reichenbach, if I have understood him rightly, claims to have developed a method for complete elimination of indexical expressions (p. 287). Since this method is very intricate and interwoven with his peculiar theory of token-quotes, a criticism of which is beyond the limits of this paper, I shall state only somewhat dogmatically that a thorough investigation of this method has shown me that an elimination according to it requires additional knowledge on the part of both producer and recipient, in conformity with our former results.

With respect to Reichenbach's contribution to our whole topic, I fully agree with Nelson Goodman when he states<sup>1</sup> that "Reichenbach goes considerably beyond Russell's remarks on ego-centric particulars" but that his "treatment is still incomplete and faulty in some respects. Nevertheless, the approach is essentially correct, and there is probably no equally adequate discussion of the matter in print." (The 'is' in the second clause of the last sentence is tensed and refers, in its context, to 1947. And my agreement to Goodman's evaluation holds, therefore, only with respect to this date.)

## VII

P. F. Strawson, in a recent paper in *MIND*,<sup>2</sup> dealt with many of the topics discussed here, and I believe that our evaluations

<sup>1</sup> In his review of Reichenbach's *Elements in Philosophical Review* 57(1948), p. 102. Goodman's own treatment of the problem in his *Structure of Appearance*, Harvard University Press, 1951, ch. xi, was published too late to be discussed at this place.

<sup>2</sup> "On Referring", *MIND* lix (1950), pp. 320-344.

of the functions of indexical discourse coincide to a high degree. Strawson draws admittedly rough and ready distinctions between a sentence (understood to be always in this context short for 'a sentence having a uniquely referring expression as its subject'), a use of a sentence, and an utterance of a sentence (325). Whereas his sentence corresponds to my sentence-type and his utterance of a sentence to my sentence-token, his use of a sentence is apparently somewhat more specific than my judgment. According to Strawson, only by producing utterances of the *same* sentence, one can make the same use of this sentence. The question when two judgments are the same has been left dependent, by me, on the question of identity between propositions. And usually this identity is defined in more liberal terms than identity of the sentences referring to them. According to one of these more liberal definitions, two judgments can be identical without their first components belonging to the same sentence-type. I have no intention to enter here any more the discussion about the merits of the various possible decisions; whether the propositions referred to by the sentences 'The sun is larger than the moon' and 'The moon is smaller than the sun' should be considered as identical has already been discussed *ad nauseam*, and I have no radically new solutions to offer.

I agree completely with Strawson when he warns us of the danger of confusing talk about sentences with talk about uses of sentences, and he is right when he charges Russell with making sometimes just this confusion (329). But I do not agree with him that this kind of confusion is responsible for the alleged mistakes in Russell's theory of description. With respect to the sentence, "The king of France-in-1872 is wise", it is obvious that all its utterances—to adopt for the moment Strawson's terminology—have the same character, whether they are all true or all false or perhaps neither, that therefore all its uses are the same, and that we are, finally, entitled in this case—as with respect to any non-indexical sentence (I am not saying 'statement' because Strawson intends apparently to deny this sentence its statement-character)—to apply the same predicate to the sentence itself. Whatever the reasons Strawson may have for denying that by uttering this sentence one intends to imply—in the customary sense of this word—that there exists a king of France-in-1872, they are not justified by his analysis of indexical expressions. I personally, had I happened to utter this sentence and were I asked by somebody whether I intended to assert by this utterance that there existed

a king of France-in-1872, would have rather impatiently answered: "Sure, what else?" And since I believe, in spite of Strawson's testimony, that this is the general attitude, I see no reason to avoid the formulation that the mentioned sentence entails—in the ordinary sense of entailing—the sentence, "There exists a king of France-in-1872", and is therefore false. Whether a corresponding rule of formation is convenient, with respect to an envisaged artificial language-system, is another question which has been discussed often and competently by other authors. Incidentally, Strawson's answer to the question, "Do Russellian or Aristotelian rules give the exact logic of ordinary language?", is not: "This is a pointless question; for ordinary language has no exact logic." Instead he closes his paper with the straightforward assertion (to which I wholeheartedly agree): "Neither Aristotelian nor Russellian rules give the exact logic of any expression of ordinary language; for ordinary language has no exact logic" (344). Let me stress that I do not wish to deny that, in other cases, the predicate 'pointless' might be more suitable than 'false', though final judgment on this matter will have to await a much more elaborate treatment of this predicate.

### VIII

A clear understanding of the functioning of indexical expressions can be helpful both in avoiding pseudo-problems and in solving genuine philosophical problems (though the borderline between these two cases is somewhat vague). I shall give just one example for each of these two types of application.

Sentences like

(6) I believe he has gone out, but he has not  
or like (5) ("I am dead") have given headaches to some philosophers and the discussion of the so-called "pragmatic paradoxes" connected with them is still going on. On the other hand, there is nothing mysterious in sentences like

(7) Dick believes Bill has gone but Bill has not  
or

(8) G. B. Shaw is dead in 1949.

G. E. Moore<sup>1</sup> has tried hard to destroy the mystery surrounding the sentence (6), but lack of appropriate terminology made his

<sup>1</sup> In "Russell's 'Theory of Descriptions'", *The Philosophy of Bertrand Russell*, The Library of Living Philosophers, vol. v (1944), pp. 177-225 and in "A Reply to my Critics", *The Philosophy of G. E. Moore*, The Library of Living Philosophers, vol. iv (1942), pp. 542-543.



task very difficult. Since I have already discussed once Moore's attempt at length,<sup>1</sup> I shall now show only how the new terminology will allow us to dissolve the puzzling situation simply and effectively.

Let us notice that though the sentences (7) and (8) look completely innocent, certain assertions of tokens of them would be puzzling, *e.g.*, an assertion of a token of (7) by Dick himself or of a token of (8) by G. B. Shaw himself. In the second case, we could have explained Shaw's *prima facie* strange assertion as one of his jokes and would perhaps have investigated the situation a bit more to find out what exactly might have provoked such a joke. In the first case, however, knowing nothing specially about Dick, we would be seriously puzzled and if not able to find a rational explanation for Dick's behaviour might be entitled to call it *absurd*. And that is all that is to be said in this situation. Many people behave absurdly sometimes. But it is the psychiatrist's business to deal with their behaviour and not the logician's. But is it not different with respect to (5) and (6)? Only very few exceptional assertions of (8) are absurd, but *every* assertion of (5) (which cannot be explained away as an "apparent" assertion) is absurd. Is therefore not (5) paradoxical to a degree that (8) is not? Our answer is, of course: No, not at all. That *all* assertions of (5) are absurd is due to the peculiar character of 'I' in this case, a character which has to be studied and understood much more than it has been done so far; philosophers have spent too much time in studying instead the I.<sup>2</sup> But there are also non-indexical sentences *all* assertions of which are absurd, *e.g.*:

(9) Nobody has ever uttered a sentence-token.

Let us summarize the discussion of this section: There are false statements (like (9)), all assertions of which are absurd; there are false statements (like (8)), some assertions of which are absurd; there are indexical sentences (like (3)) which as such are neither true nor false but such that some judgments containing them as first components are (possibly) false; there are indexical sentences (like (5)) which as such are neither true nor false but such that all judgments containing them as first components are false and all assertions of them absurd. (In our terminology, an assertion is a kind of behaviour and a

<sup>1</sup> In "Analysis of 'Correct' Language", *MIND*, 55, 1946, pp. 333-338.

<sup>2</sup> Cf. Gilbert Ryle, *The Concept of Mind*, London, 1949, ch. vi, pp. 186 ff.

judgment a pair of a sentence and a context.) That such linguistic situations arise is interesting enough and deserves careful study, but there is nothing in them which should disturb the logician's mind and make him start worrying about "pragmatic paradoxes".<sup>1</sup> (I do not intend to deny that there might exist situations of a different kind which deserve the time-honoured title of "paradox".)

## IX

As an example of a solution of a genuine philosophical problem I shall now give a *proof of the impossibility of any strictly phenomenalist language*. I am not the first to try to show this, but I believe that my approach is the simplest. To prove that the expression 'phenomenalist language' is a *contradictio in adiecto* or, more precisely, that the property expressed by it is logically empty, I have to start, of course, from the meanings of the two components of this expression. I believe that the partial meanings from which I shall derive a contradiction are standard. If I am mistaken in this belief, then my proof will hold for only such interpretations of 'phenomenalist language' which exhibit these partial meanings.

Now, I think that for a system of expressions to be called a *language*, as this term is commonly understood, it is necessary, though not sufficient, that logical relations such as derivability, contradiction, etc., should hold between at least some of the sentences of this system. I believe, in addition, that for a language to be *phenomenalist*, as this term is commonly understood, every sentence must contain essentially, *i.e.* uneliminably, at least one indexical expression and therefore be an indexical sentence. Since no indexical sentence is a statement and since logical relations, as commonly understood, hold only between statements (or between the propositions referred to by them) and not between other types of sentences (at least not between other types of declarative sentences; whether or not logical relations obtain between non-indexical commands, for instance, is an issue to be settled by decision—I mean, there are important

<sup>1</sup> As a paradigm of the queer arguments to which philosophers have been led by misunderstanding the functioning of indexical expressions can serve the discussion on "pragmatic paradoxes" which took place recently in *MIND*. Cf. D. J. O'Connor: "Pragmatic Paradoxes", *MIND*, 57(1948), pp. 358-359, L. Jonathan Cohen: "Mr. O'Connor's 'Pragmatic Paradoxes'", *MIND*, 59, 1950, pp. 85-87, and P. Alexander: "Pragmatic Paradoxes", *MIND*, 59, 1950, pp. 536-538.

relations between commands which require study by logicians, but whether they should be called 'logical' or some other term is a verbal issue—and of no importance for my proof), no logical relations can hold between any sentences in a phenomenalist language, which concludes my *reductio ad absurdum*.

*Hebrew University, Jerusalem*

## VI.—DISCUSSIONS

### INDIVIDUALS AND PROPERTIES

In a recent most interesting and stimulating paper (MIND, No. 244) Professor Ayer draws a new distinction between individuals and properties in order to tackle some old philosophical problems. Although I agree with much of the spirit and the letter of his paper I shall in the following remarks propose a distinction which is somewhat different and seems to me to be more fruitful. It applies not only to empirical, but also to logical properties; it draws attention to philosophically important ambiguities in the notion of individuals and thereby illuminates the controversy whether and, if so, in what sense, sense-data, physical objects, biological species, unobservable particles, numbers, etc. are individuals. It lastly helps us to see more clearly some philosophical problems which, in spite of some appearance to the contrary, are left untouched by logical distinctions of the type to which both it and Professor Ayer's distinction belong.

#### I

Like Professor Ayer I shall, sometimes at least, speak of indicators and predicates rather than of individuals and properties. If all parts of a sentence which are not themselves sentences are called "terms", then indicators, *e.g.* "he" or "John", and predicates, *e.g.* "white" and "internally consistent" are terms. Some terms, *e.g.* "hullo" and "or", are neither indicators nor predicates.

A term may or may not have a referential use. Thus "white" and "he" have a referential use or are, as I shall say, referential terms while "hullo" and "or" are not referential terms. Again a term may or may not have a use as the antecedent or the consequent in a statement of logical deducibility. The term "white" for example is so used in the statement that "white" (or "being white") entails "coloured" (or "being coloured"). The term "or" is not used in this way. It is not, as I shall say, an inferential term because it may, roughly speaking, be used *in* but not *as* the antecedent or consequent in a statement of logical deducibility. While no more need be said here about inferential terms the notion of reference will be discussed in more detail at a later stage of the argument.

The following classification of terms is convenient for my purpose: (a) terms which are neither referential nor inferential; (b) terms which are inferential, but not referential; (c) terms which are inferential and referential; (d) terms which are referential but not inferential. I define indicators as referential but not inferential

terms and predicates as inferential terms. Thus demonstrative pronouns and logically proper names are indicators while the definition of predicates covers both empirical predicates and logical predicates such as "entails".

The definition of predicates does not depend on a definition of reference. But the question of the range of the term "indicator", i.e. of the range of individuals, and the question whether, and if so, in what sense all predicates are referential, do depend on a definition of reference. In leaving these questions open we pave the way for attacking some difficulties which are not tackled in Professor Ayer's paper.

Before turning to these difficulties I shall show briefly that the answers given to Professor Ayer's questions on the basis of the new definitions, are similar to his own answers. Thus indicators are not "short-hand" predicates since they are not predicates of any sort. Again, using our definitions and any of the usual notions of logical equivalence, we must admit that it is not possible to "find for every descriptive statement which contains an indicator a logically equivalent statement in which all the non-logical signs are predicates". Lastly, we can agree with Professor Ayer that "a language which contained only predicates as descriptive signs (could) describe all the facts that our language does . . ." since "we might be lucky or skilful enough to be able to communicate solely by means of indefinite descriptions . . ."

## II

I now turn to philosophical questions the answers to which depend on a closer characterisation of the notion of reference. A definition of "individual" and "indicator" would not be satisfactory unless it implied that whatever can serve as an example in an ostensive definition (of an ostensively definable predicate) is an individual. Thus whatever is used as an example in an ostensive definition of colour- or physical-object words is an individual.

By adopting the above mentioned minimal characterisation of individuals as possible examples in ostensive definitions we leave room for a more detailed and precise determination of the nature of these definitions. Yet even without such further determination many *prima facie* individuals such as numbers, biological species, unobservable atomic nuclei and numbers cannot be regarded as individuals.

The definition of individuals may be widened by defining them as instances of predicates which, although not ostensively definable, are used in accordance with rules to which ostensive definitions could be added without inconsistency. According to this more liberal definition atomic nuclei, which might become observable and thus examples in ostensive definitions, but *not* also numbers and geometrical triangles would be individuals.

By a further widening of the definition we might admit instances of predicates governed by rules to which an ostensive definition could not be added without inconsistency, provided that the predicates fulfil certain conditions, *e.g.* stand in specified relations to ostensively definable predicates.

If for some reason or other we adopt a narrow definition of individuals, *i.e.* of what is referable by indicators, then we must admit non-referential predicates—a possibility left open by our definition of predicates as inferential terms. The use of non-referential predicates, just as the use of terms which are neither referential nor inferential, could be explained after the fashion of a Russellian or similar theory of contextual definition. I can see nothing monstrous in the notion of non-referential predicates, and indeed logical predicates are frequently so regarded to avoid postulating instances of predicates which are far removed from ostensively definable ones.

Throughout the history of philosophy the controversy about individuals and universals has found expression in violent assertions and denials of the individual character of numbers, species, etc. Those arguing *e.g.* for or against the view that numbers are individuals do not simply appeal to an allegedly agreed definition of individuals. Neither do they claim to record or to propose a use of the term "individual". Their concern is to weigh from their different stand-points the *reasons for adopting* one rather than another of its uses or one rather than another modification of its uses. The disputing philosophers are not like jurists discussing the application of a law or the various possibilities of filling a gap in their country's legal system. They are rather like members of a legislative assembly discussing the best way of filling this gap. A definitional decree is thus of no use to the philosopher who wishes to decide for a definition, and although the logician by distinguishing clearly between various possible definitions can give much help he cannot solve the problem.

### III

Professor Ayer has shown that in describing the facts which our language describes we could manage without indicator-words. He has not and would not, I believe, claim to have shown that we could manage without indication. This latter, and much more exciting, thesis seems false to me for reasons which can be sketched briefly. The "facts which our language describes" include facts described by ostensively definable predicates. Although not all ostensively definable predicates need be so defined (since they may be definable in terms of other such predicates) *some* of them must be defined by indicating examples, whether this be done by indicator-words, by pointing, by telepathy or by any other means.

In any case the difference between the statement that we can manage without indicators and the statement that we can manage without indication is of great importance since many far-reaching philosophical doctrines are incompatible with the latter, but compatible with the former statement. To give one example, the view, urged by Kant against Leibnitz, that there are "two stems of human cognition . . . namely sensibility and understanding" is compatible with the view that we could manage without indicators and incompatible with the view that we could manage without indication. By overlooking this we should incur one of the constant dangers of the otherwise profitable return-journey from the material to the formal mode of philosophising: the danger of forgetting some of our unsolved problems on the way.

S. KÖRNER

*University of Bristol*



## PROFESSOR RYLE ON ARGUMENTS AND INFERENCE LICENSES

IN "*If*", "*So*", and "*Because*",<sup>1</sup> Professor Ryle discusses "the major differences between our theoretical uses of "if-then" sentences, and our theoretical uses of "so" or "therefore" sentences, as well as the connexions between them". His theses are, first, that "if-then" sentences, when put to these uses, should be regarded by logicians as formulating *inference licenses*; and, second, that inferences or arguments in accordance with a corresponding license are logically related to the later as *applications* of it.<sup>2</sup>

This way of putting the roles and relations of the two types of sentence in their theoretical use seems to me an illuminating one generally. But some of the observations made by Ryle in the course of the article in question suggest that he has not completely assimilated his own major contention regarding the peculiar logical status of the licensing hypothetical. This is true in particular of his claim that "if p then q" makes a statement, while "p so q" does not. Let me try to show, by examining his reasons for saying this, the incoherence it imports into his discussion of how these expressions should be analysed.

In arguing that "if p then q" is a statement, Ryle considers four groups of criteria for deciding whether an expression is used to make a statement. Two of these—that its verbs are in the indicative mood, and that it can be used to answer a corresponding question—are dismissed as inadequate. Let us look briefly at what he says about the other two.

The most important criterion Ryle considers to be suitability for the "premissory job". We must agree that this successfully rules out "p so q"; Ryle rightly claims (p. 326) that although "the conclusion of one argument may be the premiss of another argument . . . an argument itself cannot be the premiss or conclusion of an argument". The question remains, however, whether "if p then q" is any better qualified for the premissory job. It seems to me that insofar as we follow Ryle in interpreting the hypothetical as an inference license, it is *no* better qualified. For if a hypothetical serves as the premiss of an argument, it *ipso facto* forfeits the logical status of an inference license.

It is arguable that there are two quite different ways in which an expression of the form "if p then q" may be used. The distinction between them could be brought out by asking whether we

<sup>1</sup> *Philosophical Analysis*, ed. Max Black, pp. 323-340.

<sup>2</sup> I shall not discuss what Ryle says about "a third class of theory-constituting sentences" of the form "q because p".

intend to point out merely that it is generally found that . . . , or rather that we are justified in concluding that . . . .<sup>1</sup> Used in the latter way, the hypothetical exhibits our authority for arguing "p so q"; used in the former way, it states a fact (or at any rate purports to). There need be no surprise that one form of expression may have more than one logical function. But since there is nothing in the 'look' of the hypothetical sentence to indicate which way it should be interpreted in particular cases, i.e. which of the above two rubrics would be appropriate, we must be wary of slipping unconsciously from one to the other in the course of a discussion. The two possible interpretations are *alternatives*; any given "if-then" expression *might* be used either way, but not both at once.

Yet this, it seems to me, is just what Ryle tries to do. He wishes to interpret theory-constituting hypotheticals in the licensing way, while at the same time insisting that such hypotheticals are "statements". In order to certify the hypothetical as a statement under the present criterion, however, he must ignore what he himself (p. 328) says "the Tortoise proved to Achilles", and thus take the first step along a regress which, if pursued, would deprive a corresponding argument of all validity. I conclude that if an "if-then" expression is to be interpreted as formulating a license to infer, it is *not* suitable for the premissory job, and hence is not a statement according to Ryle's first criterion.

His second important criterion is, that if an expression is used by someone to make a statement, "it is always or usually appropriate to ask whether he knows, believes or supposes that it is the case . . ." (p. 325). Once again, expressions of the form "p so q" are clearly ruled out, for the question is virtually unintelligible if seriously asked of the argument itself, rather than of its conclusion. But it should be clear that the same unintelligibility results from asking this question about "if p then q", insofar as it is taken as expressing a license to infer. Once again, Ryle fails to pursue to its logical conclusion his own doctrine of the licensing role of the hypothetical.

Of course, we *can* speak of "knowing the principle of an argument", but—as Ryle himself argued in his earlier, well-known distinction between knowing how and knowing that—this is knowing how to do something, rather than knowing that something is the case. It is interesting, in this connexion, to notice that later, in the part of "*If*", "*So*", and "*Because*" especially devoted to showing the licensing role of the hypothetical and its peculiar logical relation to the corresponding inferences and arguments (p. 329), Ryle writes not of knowing that "if p then q" is the case, but of

<sup>1</sup> This way of making the distinction I learned from Mr. S. E. Toulmin. I am not sure whether Ryle would deny the legitimacy of the factual interpretation altogether, or deny just that it would be a "theory-constituting" use of the hypothetical.

"knowing if  $p$  then  $q$ ". And again (p. 334) he writes: "When I learn 'if  $p$  then  $q$ ', I am learning that I am authorized to argue ' $p$  so  $q$ ', provided I get my premiss ' $p$ '"—i.e. "that I am authorized", and not "that so-and-so is the case". It is mainly in the earlier passages where Ryle is calling upon what he takes to be the accepted criteria of "statement" (which, for purposes of this discussion, I do not question) that he slips into the *factual* interpretation of the hypothetical. (If this interpretation is legitimate—which may be a matter for controversy—it would be possible, of course, to say 'I know that if  $p$  then  $q$ ', meaning 'if  $p$  then  $q$ ' is the case.)

It might be added that the distinction here is not between knowledge that is linguistically formulable and knowledge which is not. For, as Ryle says, although knowing "if  $p$  then  $q$ " is primarily being able to argue in accordance with it, the principle will itself sometimes be formulated as an inference precept, for self-encouragement or pedagogical purposes. But it is central to Ryle's thesis that when we teach the license "if  $p$  then  $q$ ", we no more add to anyone's knowledge of what is the case than when we teach a recipe for currant buns. Thus it is *not* appropriate to ask a person who recites the inference precept whether he knows, believes or supposes it is the case. Ryle's hypothetical is thus not a statement on the second criterion either.

So far, I have contrasted the licensing interpretation of the hypothetical with a factual one, in which "if  $p$  then  $q$ " does make a statement. But there is, of course, another and broader sense in which "if  $p$  then  $q$ " can be used to make a statement without at all slipping into the factual interpretation which Ryle must avoid. This might be called the "courtroom" sense: it envisages a situation where someone is asked: "Please state your . . .". But in this sense, not only inference licenses, but arguments, explanations, views and even questions can be stated. Since one of the purposes of calling the hypothetical a statement in the first instance was to distinguish it from arguments, this can scarcely be the sense of "making a statement" which interests Ryle.

I have argued that Ryle is wrong to say that the licensing hypothetical "if  $p$  then  $q$ " makes a statement in the same sense in which " $p$  so  $q$ " does not. And I have suggested that his failure to notice this may owe something both to an obscurity in the notion of "making a statement" (which, of course, I have only begun to indicate), and to the possibility of interpreting the hypothetical in two ways. Let me turn now from asking how Ryle came to hold his view, to asking why he bothered to employ the notion of "statement" at all in the attempt to analyse the roles and relations of "if  $p$  then  $q$ " and " $p$  so  $q$ ". If we admit that his hypothetical is not used to make a statement, is anything abandoned which Ryle would be very interested in safeguarding?

As to his ulterior purposes, Ryle reveals them himself quite

candidly (p. 333). His aim is to show that asserting and following hypotheticals is more "sophisticated" than wielding and following arguments, and also to deal a blow at the view that "if-then" expressions are used to report or describe necessary connexions between facts. The first aim is achieved through the notion of "application", and it seems to me that for this, Ryle positively *requires* a hypothetical which is not used to make a statement, *i.e.* one which formulates the principle of the arguments which apply it. The second aim is intended to be achieved by showing that arguments do not assert the existence of the necessary connexions in question because, not being statements, they assert nothing at all. And since hypotheticals are really "sophistications upon inferences", whatever it is that *they* state cannot be the obnoxious necessary connexions either. Ryle thinks that traditional failure to recognize this "is partly owing to the habit of ignoring inferences (which are not statements) and concentrating instead upon hypothetical statements and explanations (which are), and partly owing to the tacit and false assumption that such statements belong to pre-inferential levels of discourse".

If showing that arguments are not statements has such a beneficial effect, surely this effect would not be diminished by showing that (at any rate *some*) hypotheticals are not statements either. If the temptation to believe in necessary facts comes from our belief that to accept "if  $p$  then  $q$ ", because it is a statement, *must* be to accept a description or report of such facts, then need we any longer feel tempted?

WILLIAM H. DRAY.

*Balliol College*

## A NOTE ON MR. HIRST'S RECENT PAPER IN *MIND*

MR. HIRST, in a recent paper 'Perception, Science and Common Sense', writes as follows: "It is characteristic of such hallucinations that the victim is not in full control of his faculties and powers of discrimination owing to drunkenness, fever, madness, starvation, or even acute anxiety and drowsiness. Hence he is not able to distinguish properly between perceived objects and mental and dream images, especially as, owing to these disposing factors, the images often have unusual vividness. Normally the mistake of thinking that images are real things is easily avoided, but the victim of such hallucinations is not in a normal state: we must, therefore, challenge that having hallucinations is normal sensing or perceiving but with a peculiar sort of object." He gives, as the medical conditions besides 'madness' in which 'such hallucinations' are found, delirium tremens and delirium febrile.

It is unfortunate that philosophers should not take the trouble to become thoroughly acquainted with their subject before writing about it. Mr. Hirst, after all, is not examining the semantics of the word 'hallucination', but gives in this passage an account of the alleged medical conditions under which hallucinations occur and what happens in them. If, however, Mr. Hirst had first read the extensive literature on the effects of the alkaloid mescaline (discovered as long ago as 1886) on the human being he would have found there a large number of instances of clear and unmistakable hallucinations occurring in the absence of any interference with the subject's powers of careful observation and 'discrimination'.<sup>1</sup> In the presence of complete clarity of mind people under the influence of mescal will describe their experiences in such terms as 'I see', or 'It is wonderful to see', or 'as I gazed', etc. On the other hand, some subjects do not have visual hallucinations but experience a great increase in the vividness of their mental images and it is possible to differentiate clearly between these two. I quote but one of many such accounts;<sup>2</sup> "It would be out of place here to discuss the obscure question as to the underlying mechanism by which mescal exerts its magic powers. It is clear from the foregoing description that mescal intoxication may be described as chiefly a saturnalia of the specific senses, and above all an orgy of vision. It reveals an optical fairyland, where all the senses now and then join in the play, but the mind itself remains a self-possessed spectator. . . . The mescal drinker remains calm and collected

<sup>1</sup> J. R. Smythies, 'The Mescaline Phenomena', *Brit. Journ. Phil. Sci.*, vol. 3, 1953, p. 339.

<sup>2</sup> Havelock Ellis, 'Mescal, A new artificial Paradise', *Ann. Rep. Smithsonian Institute*, pp. 53 f., 1897.

amid the sensory turmoil around him ; his judgment is as clear as in the normal state ; he falls into no oriental condition of vague and voluptuous reverie." Since the hallucinations induced by mescal cannot be explained by Mr. Hirst's hypothesis, the confusion, loss of powers of critical judgment and discrimination, etc., produced by other agents which also produce hallucinations would seem to be without causal significance (or any other kind of significance) in the aetiology of hallucinations or in any evaluation of their status. The confusion, etc., would appear to be coincidental side effects of these other forms of intoxication. Confusion and hallucination are thus clearly two independent variables, which though often found together, are nevertheless to be found apart. Furthermore, the hallucinations produced by mescal are the most brilliant and clearest cut of any. In fact they may be more brilliant in colour, more delicate in form, than any normal sensa. Insofar as mental confusion would play any part in hallucination, it would make them seem *less* vivid.

It would be much better if philosophers would not base their arguments on popular superstitions about psychopathological phenomena but would study the phenomena themselves and the extensive literature on their natural history. Indispensable preliminaries to any philosophising about hallucinations would be : (i) a comprehensive study of the psychopathological literature on hallucinations and the mechanisms underlying them such as may be found in the writings of Klüver, Beringer, Rouhier, Macdonald Critchley, Meyer-Gross, Schilder, etc. ; (ii) to experience some good hallucinations for oneself. If anyone wishes to take mescal it might be advisable to get in touch with someone known to have worked in this field for technical advice as its use entails certain risks in unskilled hands.

J. R. SMYTHIES.

*University of British Columbia*

## VII.—CRITICAL NOTES

*Thinking and Experience.* By H. H. PRICE. Hutchinson's University Library. Pp. vi + 365.

PROFESSOR PRICE's book contains such a wealth of interesting and valuable matter that it is quite impossible in the compass of a review even to summarize it adequately without attempting to criticise it. I shall endeavour here to indicate what I take to be the main points.

The book opens with a discussion of two different ways in which philosophers have dealt with the obvious fact that there are in the world many instances of *repetition*, both simultaneous in space and successive in time. There are many particulars which resemble each other in *more or in fewer respects* and in a *greater or less degree* in any one respect. One way of dealing with this fact is to take such resemblances as basic, and to define such phrases as 'This has the quality *q*' or 'This stands in the relation *R* to that' in terms of them. This Price calls 'The Philosophy of Resemblance'. Another way is to say that there are certain entities called 'universals' which are 'instantiated by' particulars; that one and the same universal may be instantiated by each of many particulars; and that many different universals may be instantiated by one and the same particular. The resemblances in question are then regarded as due to two or more particulars instantiating one or more universals in common, to their instantiating different determinable forms of a common determinate universal, and so on. This Price calls 'The Philosophy of Universals' (as an abbreviation for the more accurate title of the 'Philosophy of *Universalia in Re*').

After carefully discussing these two 'philosophies' Price concludes that, taken as purely *ontological* doctrines, there is nothing conclusive to be said against either. They are best regarded as two alternative ways of treating the same facts. The terminology of Universals is more handy; but it *may* lead careless or foolish persons to treat a characteristic as if it were a kind of thing, and it may make them think that the world of particulars is more neatly pigeon-holed than it in fact is. The terminology of Resemblance keeps us nearer to the basic facts; but it is clumsy and complex, and it may make us ignore the occurrence of close or even exact resemblance. These two 'philosophies' henceforth make sporadic appearances throughout the book in various *epistemological* contexts.

Chapter ii is concerned with what Price calls 'Recognition', which he regards as *the* fundamental intellectual process. He first subdivides this into recognition of a presented particular as an instance of a familiar *characteristic or species*, e.g. as red or as a cat,



and recognition of it as a manifestation of a *familiar continuant*, e.g. a certain pillar-box. We might perhaps call these two processes 'specific identification' and 'numerical identification'. Price argues that the latter involves the former and something else besides. To identify what one now sees with Mr. Jones, the presented particular must be recognised as having 'the Jonesy look'. And it must further be taken to be connected by a spatio-temporally continuous sequence of particulars of certain kinds with certain past particulars which had that look when they were presented to one. It is plain, from the behaviour of animals, that both these processes take place in them; and it is plain that they often take place in us when we do not and could not formulate our experience in words and sentences.

Price next subdivides specific identification into what he calls 'primary' and 'secondary'. It is an instance of the former when one looks at something and identifies it as *white*. It is an instance of the latter when, although one is *merely* looking at it, one identifies it as *cold* or as *snow*. Even primary specific identification presupposes that instances of the characteristic recognised have been presented to one, and that the relevant common features have made some kind of persistent impression on one. But it does not presuppose that any explicit process of comparison, contrast, and abstraction has been performed. It is to be noted that the characteristics recognised in primary specific identification may be highly *complex* and may be relatively *indeterminate*. Even a person who has the use of speech and of imagery may not be able to name or to image them. (Cf. e.g. the 'look' by which one recognises a friend.)

Secondary identification is a very special case of identification by means of *signs*. The sign-property, e.g. the whiteness of the visually presented object, is recognised *primarily*. The significate-property, e.g. the coldness or the property of being snow, is recognised *secondarily*, through the activation of an associative disposition originally formed by past conjunctions of visual and tactual sensations. One peculiarity of this form of sign-cognition is that there is no interval of felt transition (either inferential or associative) between recognition of sign and recognition of significate. Another peculiarity which Price alleges is that the significate-property must be *capable* of being primarily recognised, and must *in fact* have been so recognised in the past experiences of conjunction which gave rise to the relevant associative disposition. (This fits in with his example of a visually presented particular 'looking cold'. I do not see how it can be reconciled with his example of the secondary identification of a visually presented object as a *bit of lead*. Surely no presented object can be *primarily* identified as a *substance of such and such a kind*, seeing that the latter property involves causal and dispositional properties.)

Price then considers how the fact of primary specific identification would be dealt with (1) by the Philosophy of Universals, and (2) by

that of Resemblances. According to the Philosophy of Universals, recognition would be awareness by an individual that a particular now presented to him is an instance of a certain universal which had been instantiated by particulars presented to him on former occasions. It thus presupposes what we may call 'retentiveness', though the excitement of the trace need not, and generally does not, result in conscious *recollection* of those past particulars or *comparison* of the present instance with them as recollected. In the Philosophy of Resemblance the essential part played by retentiveness is more explicit. On that view, to identify a presented particular as one of a certain sort is to recognise that it resembles each one of a certain set of particulars, which were presented to one in the past, at least as strongly as the least resemblant of these resembled each other. (I have stated this rather more elaborately than Price himself does.) These 'exemplary' particulars must all have had a fairly strong resemblance to each other in certain respects, and as much *unlikeness* as may be in all other respects. Both 'philosophies' have to assume that there is an innate tendency to be impressed by such likenesses standing out against a background of unlikeness, and that the trace of such an impression is activated when one is presented with anything that *in fact* sufficiently resembles the exemplars in the respect in which they outstandingly resembled each other. They have further to assume that, when such a trace is activated, it can and generally does manifest itself in a feeling of familiarity and in appropriate reactions towards the presented particular, *without* calling up recollections of the exemplars.

In this connexion Price points out that, when a class has been marked out by resemblance to certain mutually resemblant exemplars, other selections from it would have done equally well as an exemplary group. He also points out, as an analogous case, that, when a person uses intelligently or hears understandingly a word like 'tall', he is plainly in some sense 'remembering' human bodies or various heights and 'comparing' the human body under consideration with them. But equally plainly he is not as a rule *recollecting* one or more of these 'exemplary' human bodies.

In Chapter iii Price discusses the question whether *primary* recognition can be mistaken. Such recognition involves two factors, *viz.* *noticing* something present, and (in some sense of the phrase) 'remembering' something past. Price says that *noticing* cannot significantly be said to be either correct or incorrect. It may be altogether absent in cases where one would expect it to be present. Or it may be partial, either in the sense of ignoring certain features which are being sensibly presented among others, or in the sense of taking note only of a relatively indeterminate characteristic and ignoring the determinate form in which it is being presented. (I suspect that critical discussion would reveal some very thin ice here.)

The fundamental question for Price is, therefore, whether 'memory', in the sense in which it is involved in primary recognition, can be mistaken. The discussion throws light on what that sense is *not*, and shows how tenuous that sense is. Price considers the hypothesis that an individual was born one second ago, but has all the ostensible memories which a normal adult member of its species would have. He regards this hypothesis as logically, if not causally, possible. Let us suppose, *e.g.* that this hypothetical individual is a cat. Then I understand Price to assert that, if it were to see what is in fact a mouse, it would be able to 'recognise' the characteristic 'mousey' look, though in fact it had never seen, or even dreamed of or imaged, anything that looked as a mouse looks. Now Price repeatedly insists that 'memory' is an essential factor in all conceptual experience and intelligent behaviour, and that this important fact has been sadly overlooked. But, since the ostensible remembering required need not be veridical, and need involve nothing more than a feeling of familiarity in reference to the presented particular, the assertion seems little more than a tautology. We should *not* talk of a presented particular as being 'recognised' *unless* it felt familiar; and we *should* talk of it as being 'recognised', *provided* it felt familiar, even though that feeling should be completely misplaced because no such particular had in fact ever been presented to that individual before.

On the main question Price's conclusion is that primary recognition is better described as *non-fallible*, *i.e.* as something below the level at which the notion of true or false can correctly be applied, than as *infallible*.

In Chapter iv Price considers the general features of Sign-cognition. (Secondary recognition is a very simple special case of this.) He mentions and describes the following four features of sign-cognition. (1) It involves two closely blended aspects, *viz.* a sensational or quasi-sensational one and an ideal or conceptual one. (2) It is in principle independent of the use of words or images. (3) It is closely bound up with relevant practical behaviour, *viz.* doing or setting oneself to do something *in reference to* the significate. (4) The reaction, overt or private, remains closely bound to the sensory or quasi-sensory experience which presents the sign on any occasion.

The sensational or quasi-sensational aspect of sign-cognition presents no difficulty. Any occurrent instance of such cognition presupposes an associative disposition, already formed in an individual by appropriate past experiences, and a present sensory or quasi-sensory stimulus to excite it. The excitant may be either veridical or more or less delusive or even completely hallucinatory.

The alleged ideal aspect needs more discussion, in view of the fact that we are here concerned with a form of cognition which is in principle independent of words and images. Price mentions the following four features in which the reaction of an individual in

reference to the significate in sign-cognition is analogous to what we should unhesitatingly call 'thinking of so-and-so' when it makes use of words and images. (i) It is liable to be *mistaken*. (ii) It is a reaction towards something which is *absent*, i.e. not as such being presented to the senses of the individual at the moment. (iii) It involves *abstraction*. (iv) Something analogous to *logical* distinctions, e.g. negative, alternative, conditional, etc., can be significantly applied to it.

The first of these features needs no discussion, and the fourth is discussed in a separate chapter. As regards reaction in reference to something *absent*, Price points out that in secondary recognition the signified characteristic is, no doubt, ascribed to the *perceived object*, but it is 'absent' in that it is not being sensibly presented at the time, as the signifying characteristic is. Again, in numerical identification, what is 'absent' is that sequence of past particulars which one takes to have filled the spatio-temporal gap between that which is now being presented and certain others which were presented in the remoter past. As regards *abstractness*, the point seems to be this. A cat, e.g., reacts in much the same way to anything that looks sufficiently like a mouse of *some* size or colour. Again, anything that looks sufficiently like a block of ice 'looks cold' to a grown-up Northern European, but within a fairly wide range it does not look to have one degree of sensible coldness rather than another. On the other hand, some sign-reactions (e.g. those of a skilled tennis-player) are most delicately adjusted to minute variations in the sign. So, if we are to use the same terminology in speaking of them, we must say that they are reactions in reference to an extremely *determinate* (though still abstract) significate.

Price discusses in considerable detail his contention that in sign-cognition the 'thought' of the significate is closely bound to the perception or quasi-perception of the sign. In the end this generalisation turns out to be unconditionally true only in the somewhat trivial sense that the 'thought' has to be *started* by actual perception or quasi-perception. In many cases the significate remains in some way 'before the mind' after the perception of the sign has ceased. The important questions which Price discusses in this connexion are the two following. (1) What are the circumstances which tend to make the *continuance* of the 'thought' of the significate independent of the continuance of the perception of the sign? (2) In what sense is the significate 'before the mind' in such cases of independent continuance?

His answer to the first question is that the favourable conditions are (i) that the significate shall be signified as fairly *remote* in time from the occurrence of the sign, and (ii) that the significate shall be of considerable *interest* to the individual. These conditions are strengthened if, in addition, the sign is only *weakly* significant. (This is a feature which is of course positively correlated with remoteness in time of the significate.)

His answer to the second question may be summarised as follows. We must first distinguish between *predictive*, *retrodictive*, and *juxtadictive* signs. What he has to say applies primarily to fairly long-range predictive signs. Suppose that an individual perceives something which he primarily recognises as an instance of  $\phi$ , and that this is for him a sign that an instance of  $\psi$  will occur after an interval. Suppose that he is interested in the occurrence of such a particular. Then his persistent disposition to recognise an instance of  $\psi$ , if he should perceive one, will be *sub-activated* and will remain in that state during the interval. This will manifest itself in certain characteristic modifications of his actual experience or his behaviour during the interval. These may be described in general terms as 'being in a state of *preparedness* for an instance of  $\psi$ '. The alternative manifestations include (a) the use of appropriate sentences or the occurrence of appropriate imagery, in creatures who are capable of speech or of image-thinking, or (b) increased sensitiveness to *other* signs which are relevant for or against the occurrence of an instance of  $\psi$ , or (c) appropriate actions or incipient actions. If the sign is only weakly significant, or if there are conflicting signs, the state of preparedness is liable to take the special form of *vigilance*, i.e. preparedness for several alternative possibilities, including the *non-occurrence* of an instance of  $\psi$ . (I must omit here Price's account of what happens in the case of retrodictive and juxtadictive signs.)

In Chapter v Price deals with what he calls 'the logic of sign-cognition'. He claims to detect, even at the level of intelligent animal behaviour, something closely akin to the notions of Negation, of Degrees of Inductive Probability, of Disjunction, and of Conditional Propositions.

The notion of Negation is bound up with the fact that no signs are completely reliable, and that many are predictive and involve a period of expectancy. Having perceived an instance of  $\phi$ , the individual is put into a state of preparedness for an instance of  $\psi$ . During that interval he has an experience which *we* might express by saying 'Not yet a  $\psi$ !' And, if the sign should have misled him, the period will end with an experience which *we* might express of saying 'No  $\psi$  after all!' It is the experience of falsification, again, which gives rise to something akin to the experience of expecting an instance of  $\psi$  with various *degrees of conviction*. Then, as we have seen, the significate is often relatively indeterminate, and perceiving the sign puts the individual into a state of preparedness for *this, that, or the other* alternative.

Price devotes most attention to the case of analogies to the notion of *Conditional Propositions*. The question is whether at the pre-verbal level there can be anything analogous to a sign  $\phi$  having as its significate something which would be expressed at the verbal level by a phrase of the form 'if  $x$ -then- $\psi$ '. Price holds that an instance is provided at the purely behavioural level by the case of

a cat watching a mouse in a state of preparedness to do ( $\psi_1$  if the mouse does  $\chi_1$ ), ( $\psi_2$  if the mouse does  $\chi_2$ ), and so on. This, of course, illustrates disjunction as well as conditionality.

As a result of an elaborate discussion Price reaches the conclusion that something closely analogous to the notion expressed by 'if . . . then' arises from a conflict-situation, where there is a *tendency* to make a sign-inference, but this is *inhibited* in one or another of certain ways, but is *not altogether suppressed*. Two important cases are discussed under the names of 'co-signification' and 'chain-signification'.

These may be enunciated as follows. (1)  $\phi_1$  alone is a weak sign of  $\psi$ , and so is  $\phi_2$  alone, whilst  $\phi_1$  and  $\phi_2$  is a strong sign of  $\psi$ . An individual is presented with an instance of  $\phi_1$  alone. He has a weak tendency to expect a  $\psi$ . This arouses the idea of  $\phi_2$ , which is *not* being presented to him in sense-perception. If it were actually being presented to him along with  $\phi_1$  he would *strongly* expect a  $\psi$ . This tendency is activated, but it is also inhibited by the fact that no instance of  $\phi_2$  is being presented to him. His state, due to this sub-activation, corresponds to what we should describe as taking the occurrence of a  $\phi_1$  as a sign for the *conditional* significate which we should express by the sentence 'if there should be a  $\phi_2$  also, then there will be a  $\psi$ '. (2)  $\phi$  is a *weak* sign of  $\chi$ , and  $\chi$  is a *strong* sign of  $\psi$ . An individual is presented with a  $\phi$ . This leads him weakly to expect a  $\chi$ . If a  $\chi$  were actually being presented to him, he would strongly expect a  $\psi$ . There is thus a tendency for him, when presented with a  $\phi$ , to expect a  $\psi$ . But this tendency is inhibited, though not altogether suppressed, by the fact that he has only a weak expectation of a  $\chi$ . His resulting state is what we should describe as taking the occurrence of a  $\phi$  as a sign for the *conditional* significate which we should express by the sentence 'if there should be a  $\chi$ , then there will be a  $\psi$ '.

Price compares and contrasts these cases with others where there is a feeling of *necessitation* in passing from the perception of a  $\phi$  to the expectation of a  $\psi$ . This feeling, he thinks, arises only when the prospect of a  $\psi$  is highly distasteful, and yet  $\phi$  is so strong a sign that the perception of a  $\phi$  makes one confidently expect a  $\psi$ . He says that both conditional signification and feeling of necessitation can arise only in a being who has what we may call a 'sense of objective reality', viz. in one who makes inferences from undoubted premisses even when the conclusions are highly distasteful to him, and omits to make them, even when highly attractive to him, when one or more of the premisses is uncertain or the sign is a weak one. He holds that this 'sense of reality' is closely connected with the presence of *self-consciousness*.

In Chapter vi (*Signs and Symbols*) and Chapter vii (*Signs, Symbols, and Ostensive Definition*) Price discusses with immense elaboration and patience the connexion or lack of connexion between *symbols* and signs (in the sense in which we have already considered them).

The discussion takes the form of a critical examination of what he calls 'The Sign Theory of Symbolisation'. So far as I can understand, the theory is that a symbol is a *humanly produced* particular, and that the relation between a symbol and what it symbolises for a person is essentially the same as that between a non-humanly produced sign (e.g. black clouds) and what it signifies for a person who perceives it (e.g. rain in the near future). If so, the relation is what Price calls 'inductive', i.e. a symbol now works as such for a person because he has repeatedly observed tokens of that type conjoined with instances of a certain other universal, so that an association by *conjunction* has been set up in him between the former and the latter. On the one side there would be certain words or sentences uttered, or certain gestures made, on many occasions in presence of an addressee; and on the other side certain overt states of the addressor's body or states of his immediate environment, which the addressee can perceive.

There are a number of *prima facie* objections to this theory, which Price develops; e.g. that some symbols function as such by mere *resemblance*; that we often use and understand descriptive sentences without *believing* in the existence of what they describe; that *individual words* have meanings, and that the meaning of a sentence is determined by those of the words in it; that the theory fails to deal with the *logical connectives* in empirical sentences, and with sentences which are *non-empirical*; and so on. Price attempts to provide, in terms of the theory, more or less satisfactory answers to each of these objections, and he is not persuaded that either severally or collectively the alleged difficulties are insuperable.

Nevertheless he rejects the theory on the following grounds. The theory looks at symbols entirely from the point of view of an *addressee*. It can give no plausible account of their use by a person in his own thinking, speaking, and writing. And, for that very reason, it is inadequate even as an account of their use in communication between intelligent waking persons. For in such communication the addressor is or has been thinking with the symbols as he produces them, and the addressee (if the communication is successful) is thinking with the symbols which are addressed to him. The Sign Theory is in the end circular. Understanding an utterance addressed to one cannot just consist in being led by it through association to expect so-and-so. For, in the first place, the addressee may perfectly well understand it without being led to expect anything in particular by it. And, secondly, when he is led to expect so-and-so by it, this presupposes that he understands it, though there is in general no temporal gap between the understanding and the expecting.

Yet the Sign Theory is based on certain important facts, and it emphasises an essential feature in the use of symbols. The truth is this. There must be some *primary* symbols, directly tied to observable particulars, if there are to be symbols at all. And this



tie can be established only if these primary symbols are used by one's neighbours in the main either *veraciously* or, if not, at least with *systematic* mendacity. A basic symbol, then, is a type of particular which fulfils the following conditions. (1) Tokens of that type must be readily producible and controllable at will by any intelligent being who is to use it as a symbol. (2) Tokens of that type must have been regularly conjoined with observable instances of a certain universal, and this constant conjunction must be such that it impresses and leaves a trace upon those who use the symbol. It is the former feature which makes the relation between symbol and symbolised *irreversible*, and which enables basic symbols (unlike ordinary signs) to be used in trains of thought which are not tied to immediate environmental cues.

This leads Price to an elaborate critical discussion of the processes by which one acquires ostensive definitions of primary symbols. He follows this up with a polemic against certain unnamed philosophers, who are alleged to hold the curious doctrine that one's knowledge of the meanings of *basic* symbols, though in fact acquired inductively by a process of trial and error, might conceivably have been acquired in some quite different way or even have existed without having been acquired at all. He discusses this *prima facie* nonsense with a degree of patience and elaboration which does equal credit to his heart and to his head.

The main points which Price makes on his own account are the following. One does not generally acquire an ostensive definition at a definite date and by a process of being taught. One acquires it gradually by a process of trial and error in one's social intercourse with others. There is often a stage of imperfect understanding and hesitant usage of the symbol. Moreover, one does not generally acquire ostensive definitions of basic symbols *one at a time*. Very often one acquires a more or less vague understanding of *several interconnected symbols* simultaneously, and one then uses one's more exact understanding of some of them to improve one's vaguer understanding of the rest.

Chapter viii (*The Imagist Theory of Thinking*) and Chapter ix (*Images as General Symbols*) are closely interconnected and may be taken together. Price begins by insisting on the following plain facts, each of which has been denied by some reputable philosophers. (1) There undoubtedly is a certain recognisable process which occurs in most people and which may be called 'imaging'. (2) Visual and auditory imaging, at any rate, presents itself as *acquaintance with objects of a peculiar kind*, which in some ways markedly resemble certain physical things or events as they appear to sight or to hearing, as the case may be. But in certain other respects (*e.g.* their spatial properties, their normal antecedents and sequels, and so on) they differ fundamentally from physical things or events. (3) Visual images do resemble pictures enough to make the description of them as 'mental pictures' far more illuminating than

misleading to anyone who is not either woefully silly or wilfully naughty. (4) Many people do use non-verbal images as symbols in certain processes of thinking or of intelligent bodily behaviour, and words would be quite unsatisfactory to them as substitutes.

The theory which Price calls 'Imagism' turns out to be an extreme form of a wider and more plausible theory which might be called the 'Replica Theory'. Imagism is the doctrine that the *primary* symbols are mental images. All other symbols are secondary. Words whose meaning can be given only by ostensive definitions are not really basic, because they are merely substitutes for images, which are the only *basic* symbols. Price argues that there is no difference in principle between the use of non-verbal images in thinking and the use of diagrams, models, etc. What is common and peculiar to both is that the symbols used, unlike words and sentences, are *replicas* or *quasi-instances* of the concepts which they symbolise.

Price thinks that those who find the theory plausible have at the back of their minds the following tacit assumption. They feel that there is something paradoxical about thinking of anything which is not being presented at the time to one's senses. When, and only when, the symbols used are replicas or quasi-instances of the object thought of, thinking of the absent approximates as closely as its nature permits to the ideal of inspecting the present. But this account of the motives for the theory is no justification of it. In actual fact the basic symbols are tied to what they symbolise by *ostensive definition*, without needing to be replicas.

Granted that Imagism must be rejected, it might still be the case that its adherents have given a correct account of that not inconsiderable part of our thinking which does use images as symbols. So Price proceeds to consider this account on its merits. The two fundamental tests for the possession of a general concept by a person are (1) his ability to *recognise* instances of it as such when they are presented to his senses, and (2) his ability to '*think of*' instances of it when none are being presented to him. Some imagists have held that recognition involves comparing the presented instance with a kind of standard image, which one in some sense 'carries about with one' like a pocket-ruler. Price has no difficulty in showing the absurdity of this contention.

It remains, then, to consider the imagist account of thinking in absence. The theory is that to think of an instance of a general concept, *e.g.* Dog, in absence, consists in (or at any rate involves) having an image which is a quasi-instance of that concept. Stated in this extreme form, the theory is at variance with easily observable facts. But at any rate it is true that many people often do use images when thinking in absence of an instance of a general concept. So Price proceeds to consider how images function in that capacity.

The *prima facie* difficulties may be illustrated in terms of the concept Dog. (1) Any dog-image is *ipso facto* a quasi-instance, not

only of Dog, but also of all the less determinate concepts, e.g. Mammal, Quadruped, Animal, etc., under which that concept falls. (2) Any dog-image will resemble one particular dog, e.g. a certain fox-terrier, too closely to be a suitable quasi-instantial symbol for any and every dog. (3) On the other hand, if one is thinking in absence of a *certain particular* dog (say Fido), the very best that any one image can accomplish is to resemble what Fido *looked like* on one particular occasion from one particular point of view. Price considers two suggestions (not necessarily exclusive of each other) for dealing with the first two difficulties. One is a development of a theory of Hume's, the other is the theory of Generic Images.

According to the first of these alternatives we must remember three things. (1) The question *which one* of the various concepts quasi-instantiated by a given image shall be symbolised by it on any particular occasion, depends on the predominant practical or theoretical interests aroused in the individual at the time. (This suffices to show that Imagism is not a *complete* and self-contained account even of image-thinking.) (2) The symbolisation need not take place by means of a *single* or a *static* image. There may be a *sequence* of appropriately dissimilar images, or there may be appropriate *continuous variation* in a single persistent image. (3) In most cases such developments will not actually take place except in a very scrappy form. A more accurate account of the facts is to say that the individual is in a *felt state of readiness* to develop his actual imagery in certain directions and to inhibit developments in certain other directions.

Price considers two alternative forms of the Generic Image Theory. One is a variant of Galton's 'composite photograph' theory. The other is much more startling. Price suggests that there may be images which are *intrinsically* indeterminate in character. Some images, he says, may be 'inchoate entities, incompletely determinate particulars'. At a later stage he goes further and says: 'It would appear that such incompletely determinate particulars do occur in image-thinking . . . .' (I very much doubt if he has produced any adequate evidence for this stronger statement. But I must confess that when I inspect many of my own images I find it impossible to describe their nature in language that does not sound absurd. This causes me no surprise, since it is obvious that ordinary language was evolved to subserve utterly different ends.)

I will take together the last two chapters, viz. Chapter x: *The Classical Theory of Thinking*, and Chapter xi: *Concepts and their Manifestations*, since they are closely interconnected. According to the Classical Theory the essential feature in thinking is *inspecting* and noting relations between certain *non-sensible* entities of a quite peculiar kind, viz. 'universals' or 'concepts' or 'abstract ideas'. This doctrine is quite compatible with Empiricism, which is a theory

as to the *origin* of concepts in the *dispositional sense*, and not a theory as to what happens when such a disposition is activated. Nor does it necessarily involve Realism. For one form of it, *viz. Conceptualism*, denies universals, both *in re* and *ante rem*, and holds that thinking is concerned with *intra-mental* entities of a peculiar kind. We must therefore consider in turn the realistic and the conceptualist forms of the Classical Theory.

The realistic form of the theory is this. To say that a person has the concept Dog, *e.g.*, means that he has acquired the capacity to apprehend, in the absence of presented instances of it, the universal which is common and peculiar to dogs. He is really *thinking* of dogs when and only when he is actually apprehending that universal. A person who uses the word 'dog', however correctly, without doing this is just talking without thinking. Of this theory Price says that there is certainly no introspective evidence that we have any such experience when thinking of dogs in the absence of presented instances. And 'how very odd to suggest that being-a-dog is something which can be inspected . . . *by itself* in the absence of objects which are characterised by it'.

The conceptualist form of the theory presupposes the Philosophy of Resemblances. It alleges that the disposition, which is set up in a person who has observed a number of creatures which strongly resembled each other in certain respects and were very dissimilar in other respects, manifests itself in the following way when it is activated in the absence of anything which resembles these exemplars as closely as they resembled each other. It manifests itself as an experience of *inspecting* an *intra-mental* entity of a peculiar kind, called an 'abstract idea of a dog'. Price points out that, on this view, there would be as many abstract ideas of a dog as there are occurrences of a thought of a dog when no dog is present to the thinker's senses. He does not consider this to be a conclusive objection. (I should have thought that it is almost impossible to make sense of, *e.g.* Locke's account of mathematical knowledge, when this consequence of Conceptualism is recognised.) In the end Price rejects the theory because, if he introspects when thinking of dogs, triangles, etc., in the absence of presented instances, he simply cannot find himself inspecting any object of the kind alleged.

What makes the Classical Theory plausible is the fact that thought 'overflows symbols' in various ways. We can recognise the following three stages in thinking of a topic. (1) Thinking out a problem for oneself for the first time. (2) Carefully arranging and formulating one's thoughts about it, either for oneself or for others. (3) Repeating and applying the thoughts when they have become very familiar. Symbolism tends to be very scrappy both at the first stage and at the third. The scrappiness at the *first* stage cannot be explained in terms of habit and 'telescoping', and yet this is the stage of thinking *par excellence*.

Further *prima facie* support for the theory is derived from the following two facts. (1) We often have the experience of *groping* for the right symbols for our thoughts, and unhesitatingly rejecting some as unsatisfactory before we have found any that seem satisfactory. (2) The number of symbols which can be perceived or explicitly recollected within any one specious present is very much smaller than the minimum number needed to symbolise any coherent thought-content. Yet one's understanding of what one hears or reads, and one's production of appropriate words at each successive moment in speaking or writing, plainly depends in some way on having the whole context in one's thoughts.

The Classical Theory would account for these facts by alleging that in all such cases the thinker is inspecting a pattern of inter-related universals (or alternatively of inter-related 'abstract ideas'), and that it is this which enables him to reject unsuitable symbols, to grope after suitable ones, to hold the thread of a long discourse in his mind, and so on. Price accepts the facts, but he cannot, for the reasons already stated, accept the theory. He proposes an alternative explanation, in terms of the sub-activation of associated dispositions giving rise to a state of *felt readiness* to speak or write in certain ways, to produce or inhibit certain images, and so on. He thinks that the Classical Theory shares with Imagism the prejudice that there is something 'fishy' about thinking of an instance of a concept when none is present to one's senses, and the narrow view of 'memory' which would reduce it to recollecting formerly perceived instances. The result is that thinking is forced into the mould of *visual perception*, as that experience presents itself to naïve persons who know nothing of the physical, physiological, and psychological processes involved in it.

Price concludes his book with a most interesting detailed positive account of the various ways in which the possession of a concept by an individual may manifest itself. He considers that the *minimal* manifestation is *recognition* of an instance as such when presented to one's senses. In the absence of a presented instance a concept may manifest itself in a great number of ways. They range from sign-cognition and sign-guided behaviour; through image-thinking and the actual production of public *quasi*-instances, such as diagrams or models, or of *complete* instances; to the production of intelligent discourse and the understanding of the discourse of others. This last manifestation reaches its zenith when the individual is ready to express his own ideas, or to formulate the ideas of others, in alternative sentences to those which he actually utters or actually hears or reads.

I have been able to give here only a bare outline of the main doctrines of Price's book. There is a wealth of admirable discussion on points of detail which I have had to leave untouched. The book seems to me to be an extremely good one. It treats in a most illuminating way topics of fundamental interest and importance.

A delightful feature of it is the fairness and the thoroughness with which Price states and discusses doctrines which he eventually rejects, and the way in which he brings out the strong points in them and shows what has made them acceptable to men at least as intelligent and as truth-loving as ourselves and our contemporaries. It is a pleasure to read a contemporary philosophical book written in the English of a gentleman and a scholar. Price, as one might expect, keeps a happy mean between pedantic technicality, on the one hand, and, on the other, that vulgar colloquialism which nervously shuns every word and phrase which would not naturally occur in the conversation of one's bedmaker or one's bookmaker.

C. D. BROAD.

*Cambridge University*

---

*The Philosophy of Science. An Introduction.* By STEPHEN TOULMIN. London: Hutchinson's University Library, 1953. Pp. 176. 8s. 6d.

IN this book Dr. Toulmin discusses a small number of related and important questions that arise in the logical analysis of theoretical physical science. Indeed, his main concern is to examine the status of laws of nature in the light of their functions in physical inquiry; and the views he advances on other matters (among others, the nature of theoretical discovery, the relation of theory to observation, the role of models, the existence of sub-microscopic entities, and the justification of induction) are largely corollaries to the conclusions reached on that central issue. The over-all approach adopted, with its strong emphasis on the "language-shift" that is alleged to be involved in any new physical theory, is heavily indebted to Wittgenstein and writers influenced by the latter; and the answers supplied to the chief problem discussed are highly reminiscent, though probably arrived at independently, of the instrumentalist conception of scientific theory developed in America by Charles Peirce and John Dewey. The major claims are supported by many apt and carefully worked out illustrations drawn from the history of physics; and though the argument is in consequence sometimes repetitious, the use that is made of the examples is genuinely illuminating and ties down the discussion to the actual practice of science. On the other hand, popular accounts of modern physics are severely censured for presenting theories without reference to their office in accounting for experimental facts, while the usual run of logic texts are similarly criticized for their failure to consider logical problems in the context of concrete inquiry. Moreover, an interesting and often just polemic is carried on with some prominent writers on the philosophy of science, among others

with Mill, Mach and Russell, who are charged with generating irrelevant and hopeless logical puzzles because of their confusing laws of nature with empirical generalizations. Toulmin has certainly produced a stimulating, meaty, and serviceable introduction to the subject, for he has brought into clear focus frequently neglected points in the logic of science that merit serious attention. If much of this notice is nevertheless critical of various portions of his argument, it is not because I am out of sympathy with his mode of approach or am prepared to deny outright most of his contentions; it is because in the understandable enthusiasm for his views he seems to me to overlook important qualifications to which they are subject, and to have accepted them as uniquely valid on evidence that is less conclusive than he appears to believe.

Toulmin's central claim is that physical theories or laws of nature (such as Newton's gravitational theory or Snell's law) do not function as *premises from which* deductions to observational matters are made, but are best construed as *ways of representing* phenomena, and as techniques or rules of inference *in accordance with which* conclusions concerning empirical facts may be drawn from other such facts. Indeed, theories are said to stand in no deductive relations to statements about phenomena, for they neither imply nor are implied by the latter; and in this respect alone they must be sharply distinguished from empirical generalizations (such as "Ravens are black" or "Rabbits are herbivorous") which are so related to observation and which in consequence fall into natural history rather than into theoretical science. Accordingly, what counts as a discovery in theoretical physics is not the detection of a previously unknown regularity in phenomena—this is the job of natural history—but the construction of a new *form* for interpreting and representing familiar dependencies between things. And the models which commonly serve as the covering flesh for the mathematical skeleton of physical theory, and which usually occupy the centre of the stage in popular expositions of science, are of value to physicists only because they provide an "intelligible way" of conceiving physical systems and because they frequently suggest new problems for fertile inquiry. Moreover, since no statement qualifies for the title of "law of nature" unless we are prepared to apply it at all times and in all places, it is at best misleading to regard laws of nature as "hypotheses"—as if, like empirical generalizations, they could be unequivocally characterized as "true" or "probable" at one time or in one sector of nature but not at other times or in other places. To be sure, the scope of application of a law of nature may be limited—for example, Snell's law does not apply to certain crystalline substances, and Newtonian theory does not apply to electro-magnetic phenomena. However, the customary formulation of a law allegedly never mentions its own scope, which is reserved for separate statement; and the content of experimental research in theoretical science is never codified in



formulations of law, but in additional specifications as to the scope of laws and the way laws are to be used in representing actual phenomena. In short, whether a law does apply to a given system of physical behaviour can be settled only by experimental inquiry; but if it does apply, then on pain of not being a law it must apply to any such system, irrespective of its date and place of occurrence. The traditional problem of induction, which seeks to justify the extension of a generalization from a sample to its parent population, is thus said to be misplaced in the case of laws; and *a fortiori*, the use of the calculus of probability for assigning a degree of credibility to theories is held to be irrelevant to the practice of science. In many respects, therefore, theories are claimed to be analogous to the principles of projection employed in map construction: like the latter, they prescribe a mode of representation for experimental facts without describing just what are the actual facts; and the analogy fails to be complete chiefly because, unlike principles of mapping, theories cannot be instituted once for all in an *a priori* manner, but may require to be emended in the light of their adequacy in accounting for specific facts.

How securely based are these various affirmations and negations? Toulmin's distinction between premises from which one reasons and rules in accordance with which inferences are drawn, is obviously a sound one and is canonical in modern logical theory. Moreover, as Charles Peirce noted long ago, while every argument has its tacit "leading principle" which prescribes what conclusion is to be drawn from the premises, some leading principles may be purely *formal* and require as the sole condition for their application that the premises be merely of a certain form (as in the case of the *modus ponens*), while others may be *material* so that their scope of application is limited by the requirement that the premises contain specified subject-matter terms. Peirce also saw that one or more material premises can be eliminated from an argument without destroying its validity, provided that this elimination is compensated by the introduction of appropriate material leading principles which permit the derivation of the original conclusion from the remaining premises. Accordingly, by adopting this manoeuvre, he was able to construe what ordinarily appears as the major premise in a syllogism in Barbara as in effect a material leading principle, in accordance with which (rather than from which) the conclusion is drawn. And by an obvious extension of this device he occasionally also maintained that a theory, such as the Newtonian theory of gravitation, is simply a material leading principle for deriving instantial conclusions from instantial premises. This is also the reading of laws and theories which Toulmin appears to adopt, even if he arrives at it not by a purely formal logical analysis but by a consideration of a few actual uses of laws.

Now there is no doubt whatever that laws do function as rules of inference at least on some occasions, nor that this way of conceiving

laws is a most illuminating one. What Toulmin appears to overlook, however, is that this is not the exclusively correct way of construing the import of laws, and that in point of fact this is not the explicit function which laws always seem to have. For in the first place, the above manoeuvre can be introduced in reverse, and a rule of inference can in general be replaced by a premise—provided, of course, that some rules are retained; and in the case of material rules of inference this can apparently always be done. Thus, in the Whitehead and Russell codification of the propositional calculus a rule of replacement for defined expressions is tacitly postulated; but it is nevertheless possible to dispense with the rule as an independent principle of operation by suitably enlarging the set of primitive statements. And similarly for material leading principles, such as laws and theories when these function in this manner. Accordingly, there is no logically compelling reason for employing laws and theories as material rules of inference exclusively; and the choice between doing so and using them as premises involves nothing but questions of convenience. But in the second place, theories do in point of fact frequently function as *premises* in scientific practice, and indeed this seems to be the usual way they are explicitly employed. Most if not all systematic presentations of physical theory certainly do so; and as far as casual inspection of scientific papers dealing with special experimental phenomena reveals, this is the customary use to which theories and laws are put. Despite his official view, Toulmin himself occasionally slips into orthodoxy, as when he says that “we can *infer from* the atomic theory that cadmium vapour *must* emit light of such-a-wavelength when a current passes through it” (p. 159; only the last italics is in the text). I do not think, therefore, that he has established his central thesis as to the exclusive function of laws and theories, even though he has shown that the function he assigns to them is one they sometimes do have. In much of his book he simply forgets the soundness of his own observation that “laws themselves do not do anything: it is we who do things with them, and there are several different kinds of things we can do with their help” (p. 89).

If laws and theories are construed as rules of inference, Toulmin's contention that they stand in no deductive relations to facts of observation immediately follows: for it is clear that a statement of observed fact neither implies nor is implied by a rule of procedure. But if the comments of the preceding paragraph have any substance, so that laws and theories do frequently serve as premises, this contention must be established on independent grounds. Now it is perhaps a common-place that even when a physical theory is taken to be a *de facto* empirical statement, no set of observations ever does entail it; and Toulmin rightly criticizes Mach and the phenomenologists for holding that a theory is simply a compendious summary of empirical data, or that a theory is in principle translatable into a class of directly verifiable statements. But how

about the converse claim, on the hypothesis that a theory is not functioning as a technique of inference? Now if this contention is construed literally, and is understood to mean that a theory *taken by itself* does not imply any instantial conclusions, the claim is undoubtedly correct: for since a theory is a statement (or a set of statements) having the form of a universal conditional, the contention is simply a logical truism. But then it is doubtful whether any responsible thinker has ever asserted the contrary, except perhaps when using language carelessly or elliptically. On the other hand, if the claim made is that even when a theory is supplemented by specific statements of initial and boundary conditions, such a *conjunction* of premises does not and cannot entail instantial conclusions, it is patently mistaken. For is not just the reverse of this claim exhibited when Newton's laws are used, together with other premises, to predict the trajectory of a bullet, or when the laws of geometrical optics are employed, in conjunction with special factual data, to account for the operation of a lens?

There are, to be sure, important and difficult problems concealed in this formulation: for the expressions contained in a theory are associated with no straightforward empirical (or "operational", in Bridgman's sense) reference, while the supplementary instantial and boundary conditions do have such reference. In consequence, if the inference to instantial conclusions is to be accomplished, suitable (though frequently unformulated and generally highly vague) co-ordinating definitions or semantical rules must be introduced, which correlate relevant terms of the theory with empirically identifiable things. Possibly it is because such co-ordinating definitions must be supplied before a theory can be used as a premise in predictions and explanations of concrete happenings, that Toulmin is led to deny that a theory stands in any deductive relation to observations. But if so, he has not made the matter plain, and he has left to his readers the task of unravelling for themselves the apparent mystery of how theory and observation are connected. At any rate, his comments on the relation of maps to the geographical statements that can be read off them require a similar emendation or amplification. For according to him here also the relation is not a deductive one—just what it is he does not explicitly say—and here too this claim is either trivial or mistaken. It is trivial, if it maintains that merely from the map alone, unaccompanied by any rules of interpretation (co-ordinating definitions or semantic rules), no geographical statements follow—though whether tracings on paper without rules for interpreting them can properly be called a "map" seems doubtful. It is mistaken, if such rules are associated with the map; for example, if the customary rules are adopted, and one marking on a road map is to the right of another, it does follow deductively that the place represented by the first marking is to the east of the place represented by the second. If Toulmin were right in his claim, it apparently would

*never* be correct to employ geometric diagrams, algebraic formulae, or special logical symbolism in establishing the deductive consequences of English statements; and this seems to me to be a clear *reductio ad absurdum* of his contention.

Toulmin devotes some interesting pages to the perennial question whether sub-microscopic entities (ostensibly postulated by many physical theories) exist. He argues persuasively that "the real question" about their existence boils down to the question "Is there anything to show for them?"; and he eventually concludes that in terms of criteria generally accepted by scientists (e.g. cloud-chamber photographs) there is reason to hold that some (though presumably not all) theoretical entities do exist. However, he declines in general to label as "non-existent" (and thereby as discredited or fictional) every entity for which such "showing" is not available—on the ground that notions ostensibly referring to such entities may nevertheless possess great explanatory fertility. These are judicious and on the whole enlightening comments, even if they leave much unsettled. For example, there can be no serious doubt that the phenomena taken by scientists as "showing" the existence of theoretical entities constitute significant evidence for the latter *only if* the theory postulating these entities is acceptable on other empirical grounds. It is therefore not at all clear why Toulmin believes that the question of the acceptability of a theory and the question of the reality of its theoretical entities "may be totally independent" (p. 139). But the real puzzle raised by this discussion is how Toulmin can engage in it at all and reach the conclusions for which he argues. If the main burden of his book is sound, so that laws and theories function in scientific practice exclusively as modes of representing phenomena and as techniques of inference, what sense does it make to ask whether theoretical entities really exist and have a status beyond that of conceptual links in complex rules of procedure? Indeed, commenting in a later chapter on the use of models, he declares that though physicists regard the objects of their study as "articulated structures", there are few natural objects of this sort which fall outside the province of biology. "It is in accounting for the behaviour of systems which are in fact not articulated", he writes, "that the physicist has to look for as-it-were connexions, as-it-were structure, and as-it-were mechanism. Systems which are not in fact articulated structures are just those that he has to *regard as* articulated structures" (p. 166). I waive the question whether Toulmin has cogent grounds for maintaining that one cannot properly think of a cylinder of gas as a box full of fast-moving billiard balls, unless one knows that it is not in fact such a box. But if he has such grounds, how can he consistently argue that in terms of the physicists' criteria there is respectable evidence for the "real existence" of fast-moving molecules in a gas? If a law of nature may "almost as well" be called "a law of our method of representation" (p. 30), how can

a problem arise concerning the real existence of anything stated in that method—a problem for the resolution of which specialized training, much effort and experimental ingenuity, and in modern times large fortunes are required? In any event, it is not easy to make compatible Toulmin's official doctrine about the nature of theory, with his readiness to participate in a discussion that is intelligible only within a quite different perspective on the subject.

While the distinction Toulmin draws between laws of nature and empirical generalizations—and correspondingly, between physical science and natural history—undoubtedly has a sound basis, much that he asserts about their alleged differences is open to serious question. Insofar as the distinction derives solely from his central doctrine about the function of laws, enough has perhaps been already said to raise doubts concerning the sharpness which he claims for the division. But Toulmin also declares that since the classification of subject-matter involved in empirical generalizations is essentially that of everyday speech, the principles of this classification are not subject to fundamental modification in the light of further discoveries. In consequence, empirical generalizations are alleged to face the constant risk of being controverted by hitherto unnoted counter-instances, and may frequently need to be replaced by others. Natural laws, on the other hand, are said not to be restricted to employing everyday classifications of subject-matter; and in consequence, natural laws can be maintained despite their ostensible disagreement with observed facts by an appropriate re-interpretation and reclassification of the apparent counter-instances. This contrast seems to me a gross exaggeration, and to rest on a misappraisal of the fixity of ordinary language and on the failure to appreciate how the vagueness of its terms allows for the retention of empirical generalizations formulated in it—presumptive counter-instances notwithstanding. There are empirical generalizations (e.g. "Iron rusts in damp-air" or "Children are born only as a consequence of sexual intercourse") which would probably remain unamended indefinitely, even if the facts might seem to go against them—in part because of the indeterminacy surrounding the application of their terms, in part because special circumstances and new ways of classifying familiar things would most likely be invoked to account for their apparent failure. Moreover, many distinctions first developed in the physical sciences gradually percolate into everyday speech (for example, various fragments of the taxonomy of modern chemistry and genetics), so that the generalizations expressed by their means possess a stability and a range of application that are at least comparable with those characterizing Toulmin's "laws of nature". And is ordinary language quite as lacking in logical stratification and system as he asserts? "To speak of something as a 'blackboard'", he declares, "implies hardly anything about how it will behave" (p. 52). Granting that the language of some of the special sciences is

incomparably better organized with respect to allowing systematic inferences than is everyday language, such a judgment is little short of a caricature. On the other hand, there surely have been statements in physics of alleged "laws of nature" (e.g. the "law" of the equipartition of energy) which have had to be revised in the light of further inquiry. Toulmin does not, of course, deny this, though he sees here no parallel to alterations in empirical generalizations, and baptizes the revision of laws as being merely changes in our conception of the "scope" of laws. But it is not evident that the difference in mode of description formulates a radical difference in what happens. Again, Toulmin finds an additional contrast between natural history and physical science in that the former simply seeks out regularities in phenomena, while the discoveries of the latter consist in the detection of the "forms" of familiar regularities and thereby provide us with "new ways of regarding old phenomena". However, this claim can be made good only by fiat as to what shall count as belonging to natural history and physical science respectively. Were the constant dependencies found in phenomena of diffraction and scattering—regularities that were at one time unfamiliar to common sense—the discovery of physicists or natural historians? Was the work of Faraday in establishing the hitherto unknown uniformities of electro-magnetic induction the labour of a physical scientist or a natural historian? And if Darwinian evolutionary theory, for example, is taken to belong to natural history—it seems hardly likely that Toulmin would classify it otherwise—does not natural history present us, at least occasionally, with techniques for representing phenomena and new ways of regarding them? In short, if as Toulmin seems to believe a sharp distinction can be drawn between laws and empirical generalizations, he has patently failed to draw it.

Toulmin's comments on the traditional problem of justifying induction are in my judgment substantially sound. He advances telling reasons to show once more how empty and how otiose is the Principle of the Uniformity of Nature or its current variants as either a necessary or a sufficient assumption for validating laws and theories; and he is highly perceptive and convincing in his suggestion that the Principle is most plausibly construed not as a premise from which inferences to laws may be made, but as a principle of policy or method of uniform scientific procedure. However, in the end he overstates his case here also: he treats rather cavalierly what I think are relevant though unsolved problems; and he dismisses on insufficient grounds modern attempts at constructing a logic or calculus of inductive evidence. He declares, for example, that terms like "true", "probable" and their opposites appear to have no application to laws of nature (p. 78), and that such terms can be used to characterize significantly only statements which constitute the applications of laws and theories (p. 101). But while these dicta are well-taken on his professed view of laws, to

the unconverted they read like evasions of genuine problems. Toulmin's retort to the obvious objection that surely physicists conduct experiments with the view to confirming the truth of laws, is that "Physicists never have occasion to speak of the laws themselves either as corresponding or as failing to correspond to the facts" (p. 98), and that what physicists are really doing is to ascertain not the truth but the *scope* of laws (p. 79). For according to him, "only those formulae we are ready to apply equally at all places and times qualify for the title of 'laws of nature'" (p. 100); and in consequence, though there is perhaps some sense in asking whether a given formula tentatively adopted as a *hypothesis* is true or probable, when the title is once bestowed upon a statement it seems nonsensical to ask whether the given *law* is true or probable.

But is not all this a rather transparent dodge? If one adopts Toulmin's use of "law", what is being asserted in denying the *law* of gravitation is doubtless unclear if not self-contradictory. But it is neither unclear nor contradictory to ask the parallel question whether the Newtonian formula is a law (i.e. whether the formula is applicable equally at all times and places), and if so whether its scope is universal (i.e. whether, as many physicists at one time believed, it can account for every variety of physical phenomena). And if this is so, have we escaped the *logical* problem of induction—the problem of analyzing, codifying, and perhaps even constructing standards and measures of "reasonable" or "reliable" evidence? Toulmin finds unclear what is to be understood by such statements as "The probability of the kinetic theory of gases is 17/18" (p. 112). Undoubtedly it is, since this is not a customary mode of speech. But in therefore dismissing outright such statements as irrelevant to scientific practice, he is less than fair to those who propose to *introduce* such locutions into logical discussions on the basis of careful definitions and explanations. For he appears to ignore the crucial point that such proposals do not seek to assign a probability to a law or theory *simpliciter*, but only relatively to given evidence. One can be justifiably sceptical (and I share such doubts) of the actual or likely success of attempts to specify a quantitative measure of the cogency of evidence for a scientific hypothesis. However, even if ordinary linguistic usage is taken as the criterion of intelligibility, it is not quite the nonsense that Toulmin thinks it is to say that the available evidence for a given theory (or hypothesis) at one time is more complete or better than it is at another, or alternatively that a theory is more probable relative to one set of evidential statements than it is relative to another. At any rate, important, difficult and unsolved problems for logical analysis are involved in such vague locutions; and whether or not Toulmin is right that these problems are of no concern to the practice of scientists, he has produced no good reasons for supposing that there are no such problems at all.



I have given an exhaustive account neither of the contents of Toulmin's book nor of the reservations with which, in my judgment, his views need to be qualified. But I hope I have said enough to make evident that he has written freshly, instructively, and with much insight on cardinal issues in the philosophy of science, and to persuade readers of this notice that his book is worth careful study.

ERNEST NAGEL.

*Columbia University*

## VIII.—NEW BOOKS

*Theory of Beauty.* By H. OSBORNE. Routledge and Kegan Paul, 1952.  
Pp. 220. 21s.

THIS is a serious, informed and intelligent book on aesthetics. It contains one of the best attempts to describe in general what it is to appreciate, to enjoy, a work of art that I have read. If the author fails in his intention of showing the way to the building of a 'science of aesthetics' (p. 6), it is not because he lacks power or devotion, but because the conception of such a science is a misconception. This central misconception, abetted by a certain very high, very pure aestheticism, results in a theory not unlike some other theories of beauty, and as incapable as they of yielding the unflinching criterion the author wants. But on the way to and from the proposition that beauty is the property of being an organic whole for perception, he says many more enlightening things. He has written a stimulating book on an elusive subject.

To summarise what he says on the positive side. The function of aesthetics is to provide a co-ordinated set of principles for discriminating true from untrue aesthetic judgments (pp. 7-11). This is to be done by the investigation of the notion of *beauty*, where this is understood to mean 'the proper (characteristic) excellence of a work of art' or 'the property in virtue of which works of art are judged to be good works of art' (p. 12). Works of art are assessed in many ways, and are often valued for their possession of qualities (*e.g.* social utility) other than this characteristic excellence; and much mistaken aesthetics consists in identifying beauty with some other incidental merit that works of art may have. But 'in a genuine science of aesthetics, the definition of beauty . . . must indicate a property common to all members of the field [of aesthetic enquiry] and peculiar to them . . . a property possessed in varying degrees by members of the field' (p. 21). The natural method of pursuing the enquiry would seem to be to examine putatively beautiful things and putative instances of appreciation of beauty with a view to discovering either or both of (1) a property common and peculiar to the former or (2) a psychological element common and peculiar to the latter. Both these procedures encounter the difficulty that there is bad art, and enjoyment of bad art. To exclude these would seem to involve assuming the correctness of an answer before the investigation has begun. To admit them would render its successful termination impossible. If we could find, however, (1) some 'special psychological state or activity' which was '*more or less* confined to what has traditionally been regarded by most expert and sensitive opinion as the appreciation of beauty in art' and (2) 'some objective property *more or less* common and peculiar to the objects of this special mental state', we should have escaped from the difficulty (p. 31). There follow two chapters containing fair and pointed criticism of certain theories of aesthetics and a good attempt to explain the diversity of the judgments actually made on works of art. In chapter v, the nature of the 'objective beauty-property' is settled. It must be a formal property, for the materials of different arts are different. But arrangements even of different types of sensory impressions—for works

of art are such arrangements—may have common formal features. (A difficulty is felt over literature here, but the addition of *meanings* to sense-impressions takes care of the novel and the triolet.) After some interesting discussion of the variables of music, painting and sculpture, the definition of beauty follows. It is the property of being an organic unity for perception—the greater the complexity and the organisation, the greater the beauty. The requirements for something to be a perceptual organic whole seem to be: (1) its elements do not look or sound the same when subtracted from the whole as they look or sound as parts of the whole; (2) it is 'experienced as a single perceptual unity', is 'an object of direct intuitional awareness', like the five-pattern on a die, as opposed to a heterogeneous collection of objects on a tray. A work of art is a complex organic whole, of which the parts are organic wholes. It has 'complex configurational unity'. There follows the chapter on aesthetic appreciation. Fully to enjoy what is beautiful, we have to be aware of its complexity without at the same time actively dissecting or classifying. Our attention stops at it. We do not think of it as an instance of anything, or as something to be used or of its relation to ourselves; and by this suspension of all but our purely perceptive faculties, these are so much the more intensely employed that there is sense in saying that we only then really hear or really see.

Now first, as to the function of aesthetics. The author says it is to provide principles by which one can tell whether or not it is true to say of something 'This is a good work of art'. The body of principles is to form a science; and I take this to mean that the principles will consist of purely descriptive criteria, the application of which need not involve evaluation. But I do not think Mr. Osborne really believes in the possibility of such principles. And it is clear that they are not possible. The relation between the analysis in *general descriptive terms* of a play, a picture or a poem and the judgment that it is a good play, picture or poem is quite unlike the relation between the description of, say, a wireless set or a watch and the judgment that it is a good wireless set or watch; it is even quite unlike the relation between the description of a man's actions and the judgment that he is a good man.<sup>1</sup> It is quite meaningless and empty to praise a man's morals or the performance of a wireless set, without having reasons of a certain sort; of a sort, namely, such that giving the reasons would involve mentioning, in non-evaluative terms, generally applicable criteria of excellence in men and wireless sets. But in aesthetic matters, judgment is not in this way wedded to *non-evaluative* descriptions of *general* features of the thing judged. (No doubt, except in the very inarticulate, the aesthetic 'This is good' will often be backed and amplified by observations of other kinds. But they will be of other kinds: either not non-evaluative—'exciting', 'masterly', 'brilliant', 'original'—or not general—'Look at this . . . and that'—or neither.) This is the old point that there are no specifications for a good poem as there are for a good wireless set, and—one may add—for a good man. Of course there are rules in art and literature, (Don't begin sentences with *And*), and it may be risky to break them; but one may break them in such a way that the violation is itself an added beauty. It may also be necessary to break a moral rule, or sacrifice one of the possible merits of a wireless set, in the over-riding interest of another rule or merit; but

<sup>1</sup> Cf. Stuart Hampshire, 'Logic and Appreciation' in *World Review*, October 1952. I am much indebted to this article.

the violation, the sacrifice, will not be an added excellence, but a matter for regret.

So I do not believe in Mr. Osborne's programme, and I cannot think that he does. For he repeatedly stresses the individuality of the work of art and, by implication, the impossibility of general formulae for assessment; and he explicitly opposes the application of general standards to the 'verdict of direct inspection', the 'direct aesthetic judgment' (p. 88). Yet it is in this same place that he says that such judgments 'provide unambiguous data for science' and that by their means 'aesthetics will methodically inspect and evaluate the standards of criticism which are in use'. To replace them by others not yet in use?

Next, as to Mr. Osborne's 'objective beauty-property' of 'organic wholeness', of 'complex configurational unity'. It seems to me that Mr. Osborne means more by these phrases than he says; that part of what he means by saying that an object has complex configurational unity is that it is *well worth looking at*, in the especially receptive and yet active way of aesthetic appreciation. For when one recognises a person's face or a locality or any individual thing, for which aesthetic excellence is not claimed, one may well be noticing something that passes Mr. Osborne's test for organic wholeness: i.e. one sees it as a single individual, and the parts might look different away from the whole. Of course so to see it is not *really* to see it, in Mr. Osborne's sense. There are more ways of not looking at a thing than just concealing it under a concept; a proper name will do as well. But just so might one recognise a painting, which had aesthetic merit. The difference is that one thing is worth looking at in a way the others, perhaps, are not; but Mr. Osborne's test will not tell us why. Mr. Osborne says that he knows 'no other objects of experience which have this property of organic wholeness in more than a rudimentary degree except works of art' (p. 159). But while it is plausible to say that no sequences of sounds in nature have the structural complexity of those produced by an orchestra, it is quite unplausible to say that the aspect of the sitter's face or of the chosen scene has in not more than a rudimentary degree the structural complexity possessed as richly as you please by the portrait or the landscape painting.

Why does Mr. Osborne think it so important that there should be just one objective property in virtue of which works of art are judged to be good? If there is no such property, he says, there is no science of aesthetics (p. 21). Because there is not yet a successful science of aesthetics, criticism remains merely the expression of the personal taste of the critic (p. 11). The property must be found, then, for criticism proper to start. But (a) criticism has started, and is not always just the expression of personal taste; (b) criticism of books and pictures, as we have seen, cannot be the analogue of criticism of men and wireless sets; (c) even if the analogy did hold, this would yield no reason for supposing that there must be just one merit-conferring property. The standards of excellence for wireless receiving sets are objective and scientific enough, and involve reference to several properties. And then, works of art are so diverse. Mr. Osborne mentions this diversity, but does not take it seriously; he mentions novels and plays, but his heart is divided between the studio and the concert-hall. There is, I think, no more specific word of praise which we should be prepared to apply to all works of art we admire than the word 'good'. It is hopeless to look for a common element in all the things we go on to say in amplification of this. But to

stop at this point may properly be felt to be unsatisfactory. For when we praise works of art, and not as moralists, do we not at least praise from one point of view, from which we may also admire things not primarily designed to excite this kind of admiration, or not designed at all? Is there not a singleness in the appreciation, if not in its objects? Many have thought so. And it is true that there are resemblances between enjoying a picture and enjoying a play. So there are between enjoying a play and enjoying a speech, between enjoying a speech and enjoying an argument one is following or a game one is watching. These are spectator-enjoyments. When we try to divide or classify them further, we do so in terms of the characteristics of their objects or in terms of the faculties activated. We may take as the model of *aesthetic* appreciation the case where one sense is activated, the case, say, where the eye is pleased and engaged by the organisation of visual elements (which include not merely colour and line, but, as Mr. Osborne recognises, the visual aspects of familiar things); and are prepared to extend the term to any case where we find *analogies* that impress us strongly enough; are prepared to find an aesthetic element in our enjoyment of a mathematical proof as well as in our enjoyment of music. (It is not accident, or, as Mr. Osborne seems to think, wanton vagueness, that underlies the persistently analogical character of aesthetic writing, the talk of pattern and colour and argument in music, of rhythm and counterpoint in the plastic arts.) It is true that we find these analogies, and it is this truth that encourages Mr. Osborne to seek a single measure of beauty. But if we agree to use the word 'aesthetic' in such a way as this, then it is very far from true that the characteristic excellence of the work of writers, painters, composers is solely an aesthetic matter. It is absurd to suggest that if one praises a novelist for his insight into human nature or for enriching one's awareness of general moral truths, one is importing into one's criticism elements which are irrelevant to the assessment of the novel as a novel. Mr. Forster, writing on the novel, allows 'pattern' a chapter, as one aspect among others, and as the one that 'appeals to our aesthetic sense'.<sup>1</sup> This is not the only chapter that the serious critic of the novel will take seriously. Nor are the literary arts the only ones where aesthetically impure considerations are not irrelevant to the understanding and admiration of the work of art as such. Painters may, in *their* way, address the head and the heart as well as the eye, and may not, in doing so, be using their art as the vehicle of an artistically irrelevant purpose. Those who use the language of life and letters in talking about music are not always deaf sentimentalists. In concentrating on one set of relevant analogies, Mr. Osborne misses another. His formula is not less one-sided than 'Poetry is criticism of life', for being on the opposite side.

Near the beginning of his book, Mr. Osborne says: 'Concrete judgments about the beauty of particular things are the material available as primary data to the science of aesthetics'; and, in saying this, raises expectations he does not fulfil. There is scope for metacriticism, for an examination of the way in which the vast and varied vocabulary we employ in discussing works of art is used in these discussions. We must not think that this examination could yield us the principles that Mr. Osborne desires. What general conclusions, if any, we could derive from it I do not know. But it would be worth trying.

P. F. STRAWSON.

<sup>1</sup> E. M. Forster, *Aspects of the Novel*, p. 193.

*The Philosophy of Sarrepali Radhakrishnan.* Edited by P. A. SCHILPP in The Library of Living Philosophers. New York: The Tudor Publishing Co., 1952. Pp. xiv + 883.

THIS work is of a very different type from the others appearing in the same series in that its subject is rather a religious teacher than an analytic philosopher, nor are whatever original views he may have in metaphysics expounded here. In saying this I am not disparaging his philosophical ability or the usefulness of the work. A reader will not find much of the kind of thing for which professional philosophers in the West usually look today, but this is not to deny that the book is genuinely valuable from a philosophical and not only a religious point of view. Radhakrishnan's philosophy is ultimately based in the main on a mystic intuition, but his account is not mystical in the sense of being vague. He does make clear what his views are, more effectively indeed than do many philosophers of the "modern" type, and he insists on the connexion between reasoning and intuition, claiming that "intuitive knowledge is verified by its capacity to bring coherence and harmony into systems framed by the intellect" (p. 794). The subject of a book like this is not expected to use the volume to expound his own philosophy as a whole, which he has done elsewhere, so we cannot complain that he did not apply this test here in more detail, but it is a pity that more use is not made of it by the contributors with reference to his other published writings. Radhakrishnan gives us here and elsewhere a very moving and impressive exposition of one characteristic way of looking at the world, it would be better to call it the religious rather than, as he does in places, the "idealist" way, for it does not rest on any arguments of the Berkeleyan or Kantian type. One feels that he is giving the pure essence of religion with its metaphysical implications and that this pure essence as given by him is something which the Christian can accept just as well as the Hindu. Radhakrishnan indeed has attacked "Christianity", but what he was attacking is the claim that belief in Christ is the only right way to a religious conception of the universe. It seems to me that, whether this claim of most Christians be justified or not, we have here all the essentials of a Christian conception of God without the belief in a unique revelation through Christ. Radhakrishnan says that his "supreme interest has been to try to restore a sense of spiritual values to the millions of religiously displaced persons who have been struggling to find precarious refuges in the emergency camps of Art and Science, of Fascism and Nazism, of Humanism and Communism" (p. 14), and we might surely add to this, his desire to find and make clear to others the unity between the different religions of the world. For a philosopher to reject his view offhand on the ground that it is necessarily unjustified or even meaningless would be dogmatic impertinence. After all, if Radhakrishnan and the other Eastern mystics are right, the intuitions are, even in a strict sense, verifiable, since anybody who is prepared to devote himself to the arduous training required will have them, as anybody may verify the scientist's perceptions by the easier process of looking through a microscope or telescope, but this of course leaves the majority who are not prepared to do so in a difficult position since they can neither refute nor endorse his view of the universe.

The contributors are far too numerous for me to say much about them individually. Radhakrishnan himself hardly fulfils the usual requirements of the series in giving an autobiographical introduction, but he

presents us instead with an eloquent summary of his philosophy. There are contributions both by Western and by Indian thinkers, the former being indeed slightly in the majority. It is a curious fact that on the whole the Indian contributors are less inclined to be sympathetic, though probably most of their views come nearer to Radhakrishnan's than do those of the Americans, but the articles are in general appreciative rather than critical. The one well-marked exception to this is the article of an Indian naturalist philosopher, M. N. Roy, and the attitude of the latter is too dogmatic at the other extreme to allow for a profitable discussion. England is represented by four articles on the religious side of Radhakrishnan's thought, the contributors being Dean Inge (whose short and epigrammatic essay shows that he still retains his old liveliness), C. C. J. Webb (from whom we are equally glad to hear again), A. N. Marlow of the classical department of Manchester University, and L. Hyde. Of the American contributors I found Bernard Phillips and E. S. Brightman most worth reading. The former gives one of the best defences that I have ever seen of the claim to obtain valid cognitions by "religious experience". R. W. Browning has a long article on the different types of intuition in the philosophy of Radhakrishnan, who admits that "he has drawn attention to many of the obscurities and ambiguities in my treatment of the problem of Reason and Intuition" (p. 790). Hartshorne, who compares Radhakrishnan to Whitehead, and Conger have some interesting things to say about the Absolute, and C. A. Moore on Radhakrishnan's ethics. It is to be regretted that Radhakrishnan did not see fit in his reply to explain more adequately his distinction between God and the Absolute. His specifically religious side is discussed by K. J. Spalding and Joachim Wach, the latter from the point of view of the orthodox Christian. Northrop has an interesting article in which he applies the main thesis of his large work on the relations of Eastern and Western thought, i.e. that the former is differentiated from the latter by its conception of Reality as indeterminate and yet immediately apprehended, and traces Hindu toleration to this idea which entails that different views about reality can only differ as different modes of expressing the inexpressible. I agree with Radhakrishnan himself in not finding this account very plausible. There are sufficient good reasons for toleration even on a view which does not regard differences as merely differences of symbolism, and the distinction between Eastern and Western thought is made by Northrop much too sharply. Radhakrishnan points out in reply that there is plenty of reasoning and determinate thought in Indian philosophy and plenty of intuition in the West.

It is regrettable that owing to my lack of knowledge of Indian philosophy, which allowed me to yield only with reluctance to the editor's request to write this review, I am not adequately qualified to appraise all the contributions by Indian thinkers. I can only say that besides the already mentioned article by Roy giving an account of the naturalistic tendency in Indian thought, there are articles by Swami Bhārati on "Radhakrishnan and the other Vedānta", S. K. Chatterji on "Dynamic Hinduism and Radhakrishnan", P. T. Raju on "Radhakrishnan's influence on Indian thought", T. R. V. Murti on "Radhakrishnan and Buddhism", and E. L. Hinman on "Radhakrishnan and the Sung Confucianism", the last two taking the opportunity to travel rather far afield from Radhakrishnan. The fullest account in the volume of the development, as distinguished from the content, of Radhakrishnan's own thought is given by D. M. Datta.



The last three articles, by Humayun Kabir, B. K. Mallik and A. R. Wadia, deal with political philosophy, and are very interesting, though I must admit that Mallik's article presupposes too much of his own special brand of philosophy to be really comprehensible to somebody who is not acquainted with the latter. Radhakrishnan's reply is all too brief, especially in regard to his views of caste, but he takes the occasion of Northrop's article to give a statement of his position in regard to Communism which for its fair-mindedness should be widely read. While not in the least glossing over the terrible evils of the system which prevails behind the Iron Curtain, he emphasizes what indeed needs to be emphasized again and again in the West, that Communism is not *all* evil and that its appeal cannot be destroyed till the poorest all over the world escape the material misery which they now suffer. As for Radhakrishnan's replies to other contributors, they are interesting and generally to the point, but I cannot help regretting, as I always do when reading a book of this series, how much is left undiscussed, no doubt inevitably owing to the limitations of space of even so large a volume. Yet I cannot help wondering whether it would not be a better policy to leave more room for replies by fixing the number of contributors lower than twenty-three.

A. C. EWING.

*Dominations and Powers. Reflections on Liberty, Society and Government.* By GEORGE SANTAYANA. New York: C. Scribner's Sons (London: Constable) 1951. Pp. xv + 481. \$4.50 (42s.).

THIS work of Santayana constitutes the fruit of many years writing and reflection, it being, as he tells us in the preface, the natural continuation of his earlier *Reason in Society*. In form it is a collection of many manuscripts into a whole which is generally coherent and well written. One can read this work in either of two ways: as a collection of essays, each a finished product in itself, similar in construction to his *Soliloquies in England*, or as a connected whole. Santayana writes to reveal the interplay of social powers and social dominations, their effects upon the individual, and the growth and nature of liberty, its relation to powers and dominations. The analysis is executed on three distinct but connected levels of society: the generative, the militant, and the rational.

Book One, "The Generative Order", sets the tone and the method of analysis by taking the individual as the key to understanding the growth of society and government as well as of liberty and morality. The doctrine of the psyche of his earlier works, the doctrine of an inherited but plastic inner physical force determining the needs, desires, and values of the individual, is once more invoked as a means of accounting for individual activity and temperament. He devotes a series of essays to analysing the nature of liberty, in which he seeks to show (1) that a vacuous kind of liberty is usually what is longed for by individuals and societies, but (2) that all liberty occurs within the context of a necessary servitude: to master, parent, God, or tradition; and (3) that the important question is "how far and in what direction may necessary servitude be sweetened, and vital and spiritual liberty enjoyed, under any of these dominations" (p. 69). The growth and development of the economic and liberal arts is portrayed as arising out of the needs and desires of the individual, but the liberal arts are assigned the function of providing man's relative freedom with direction and content. "In feasting the spirit [of man]

on this its congenial food [beauty and perfection], the arts liberate it from what it felt as exile or captivity, and allow it for a moment to be itself" (p. 172). The spirit or intellectual facet of man in Santayana's system is a direct product of physical forces, a factor which is not opposed to the material world but is nevertheless a component which does not find its enjoyment in the forces of matter. Even in this the most mature expression of his views on social and political questions, Santayana does not forsake his essential mysticism of the spirit. The metaphysical categories of spirit, matter, essence, and truth constitute the framework of all his thought: man as a rational creature seeks his ideal enjoyment in the realms of spirit and essence. The enterprise is not unlike that of Plato, but whereas Plato sought to bring the existing world into conformity with the ideal essence, the objective for Santayana is to come to terms with one's environment so that the spirit may be free to pursue objectives without interference. The power of matter over spirit lies not only in the biological genesis of spirit from the psyche, but also in the constant threat of matter dominating spirit. Half the battle between these two forces is won through a stoic recognition that the demands of the physical and social environment must be recognised and fulfilled. Then, through liberal arts, the spirit follows its true ideals.

The growth of society is thus the pattern delineated by individuals in their quest for the necessary equilibrium in the realm of physical and social matter, a quest propelled by the biological forces of the internal psyche and carried forward by reason and spirit. In such a quest, motivated by many conflicting values, opposition arises, wars break out, tyrannies emerge, one group seeks to triumph the other. It is this aspect of human activity which Santayana seeks to portray in Book Two, under the heading, "The Militant Order of Society". He includes under this category all individuals who have "the love of reforming the world from the total mutation that the world is always undergoing" (p. 177). It constitutes the desire to impose upon the world one's own set of values, the ideal government as conceived by the reformer. He analyses the various manifestations of such militancy, in ruling tyrannies and imperialistic nations, in the Machiavellian concept of 'realpolitik', in war, in religion, and in business enterprise. His argument against moral militancy takes two main forms. (1) He shows sympathy with the romantic myth of the noble savage, the natural man who is capable of constructing a morality by himself if left unimpeded by militant moralists. "Leave the natural man to himself and he would forget or transform your legends, fetch a morality for himself out of his heart and his social experience, and speculate not on the basis of what tales he had heard but of what facts he had encountered" (p. 203, cf. p. 210). In this connexion, the essay on the 'Disappearance of Chivalry' should be studied, for there Santayana laments today's concept of brutal, total war and praises the romantic myth of chivalrous wars and battles of the past as containing a purer, more healthy type of conflict (pp. 204-208). Modern society not only thwarts man's own ideals and objectives, it prevents him from forming a moral system of his own, and degrades him by seeking to foist upon him some alien moral domination. (2) The second argument against moral militancy consists of his invoking the doctrine of the psyche and its generative functions. Imposition of alien dominations will not succeed: "the course of events obeys the generative order of nature, and not any militant revolt, following the mirage of a humanistic or moralistic ideology"

(p. 274). If, for example, "the communist conspiracy succeeded, it would not establish anything permanent", presumably because it does not contain the ideal of all those who have or will come under its sway. Just why Santayana holds the belief that the communist ideology cannot become the will of the people is not made clear. It is at least theoretically possible that the communist ideology could succeed in changing the hearts of men. But Santayana is correct in his observation that unless the political ideologies practised by an individual are consistent and harmonious with that individual's inner desires and system of values, maladjustment will occur and with it social and individual instability. The effects of commerce and industry, like that of wars, has been to sever the individual's value structure from his routine occupations: "the free life of neither class [workers or investors] has any moral roots in their working life" (p. 263). But Santayana himself seems to demand a union between values and everyday life only because it is the only way that adjustment to one's environment can be realised, an adjustment which leaves the spirit of man free to pursue its own delights. Behind Santayana's social and political utterances hover the realms of spirit and essence as the ends for which life and society exist. "Existence is irrational and so is the love of it; and while we exist we cannot escape either. Yet a man has also a spirit capable of being speculative and disinterested whenever the pressure of hostile circumstances or vicious passions is relaxed. At such times, all things seem to speak to that spirit in its native language. Tragedies become poems; the hopes and sorrows of the world become arguments for religion. Evils remain as bad as ever for the natural man; but for the spirit in him they are transformed; what was a predicament becomes a vista, what was a puzzle becomes a truth. You have begun to live in a new way just as natural . . . and much less disturbed. This is what society loses if it discards the liberal arts, or the liberating side of religion and philosophy" (p. 291, cf. p. 364). He is anxious to avoid all barriers which prevent the realisation of a free art and a free life of the mind.

The aim of the "Rational Order of Society" is precisely directed towards those conditions conducive to a balanced and harmonious adjustment of all individuals with their social contexts so that this pursuit of the life of reason may be achieved in comfort. Santayana seeks to distinguish sharply between the rational order of society which "would liberate all human interests, especially those that being ideal and harmless do not materially trample on one another", and the rationalist reformers who "usually favour only elementary material interests and unimaginative morals, counting bread-rations but not moments of sport or laughter" (p. 296). The social reform which only looks after the improvement of such material conditions as food and housing tends to blight the intellectual activities and enjoyments of men. The criterion of a good government is defined in terms of the liberation and protection of these values. In the second book, on the militant order, Santayana defined a beneficent government as one which is "autonomous and full of initiative; it must give direction, and not receive it; the interests it discerns and pictures need not be the private interests of its members, but interests seated in their individual hearts, quickening their imagination, and enlisting their continuous effort" (p. 226). The role of reason is to achieve the adjustment, without militancy, which individuals on the generative and militant levels cannot attain. His admiration for the natural man does not preclude his denying this ideal figure the ability of forming satisfactory associations. 'Reason'

is given a Platonic definition: "not a new force in the physical world but a new harmony in vital forces" (p. 307). The correctness or incorrectness of any act is defined in terms of this rational ordering of society: "that which makes an action rational is the material possibility of carrying it out successfully" (p. 313). To accomplish this condition, reason has to realise the difficult objective of bringing conflicting individual motives into some kind of organised whole. The section on the rational order contains some careful analyses of the notion of democracies, judged in terms of Santayana's basic criterion of liberation of the liberal mind through the arts and play. A good government must be 'morally representative', must represent the vital evaluative interests of its people. But are the people themselves capable of divining their 'true' good and of bringing about its realisation? The basic problem to be solved before we judge of the worth of democratic forms of government Santayana poses as follows. "Democracy in choosing its agents is therefore faced by this dilemma: shall representatives be expected to voice the opinions or advance the schemes of their electors; or shall they be trusted, in view of their acknowledged gifts, to serve their electors by serving the best interests of all who are affected, and thereby to secure the greatest spiritual satisfaction for ourselves and our people, even at some sacrifice of what we clamour for at the moment? In a word, is a parliament a central exchange for current demands or is it an *élite* commissioned to govern justly?" (p. 389). Santayana's analysis of democracies leads him to the conclusion that self-government can be achieved only where there is strict unanimity of opinions and values. Wherever there is some difference of thought or value, "a dualism is established between that part of the soul or of the people whose will is done, and that part whose will is defeated and ignored" (p. 409). Since he does not believe we will ever have a society of strict unanimity, his own sympathies belong with the ruling *élite*. Government is made necessary because the people have not been able to govern themselves (p. 421). Good government must render actual the 'true' good of the people. In the tradition of the 'general will' school of political philosophy (Rousseau, Hegel, Bosanquet) Santayana sees the objective of good government in the realisation and maturation of the potential nature of men. A sharp division is drawn between what a given individual desires consciously and what his 'nature' requires. With this orientation, Santayana can only return the verdict of inefficient upon all forms of democratic rule, since the vote cast by the citizen expresses only his conscious desires. But how does any one, even the benevolent ruler, gain the insight required to meet the criterion of a good governor? In his earlier works, Santayana has asserted that knowledge of the psyche, the citadel of the 'nature' of man, could be known via two routes: externally, by means of observation of overt behaviour, and internally through introspection. Apparently, the latter method is ruled out in *Dominations and Powers*, since he distrusts the verbal or overt expression of desires by individuals, on the grounds that the individual cannot determine his own 'real' desires satisfactorily enough to justify his participation in government. Santayana suggest that anthropologists, men of medicine, and psychologists are the men to consult in the attempt to uncover these hidden demands of the individual.

We have, in Santayana's social and political philosophy, another expression of the intellectual's ideal, the emphasis upon government directing the activity of the governed, of bringing out the best that is in

man, and upon the ruling élite of specialists who have insight into the hidden 'nature' of man. The concept of the well-formed psyche inherited at birth, in itself a dubitable doctrine, provides him with the basis for this concept of government. But he does not provide us with any criterion which would enable us to know when the specialists had discovered the 'true nature' of man. How could the anthropologist be exempt from Santayana's charge of militancy if he insisted that he had this special knowledge and could bring society to the level of the rational order? As far as *Dominations and Powers* goes, no criterion of this sort is offered. We can agree with Santayana's general outline of the ideal government, but like all such rationalistic systems of politics, we are left to flounder over the difficulties which it engenders. That the ideal may not be obtainable does not cause Santayana any alarm, since he does not believe any social organisation is lasting or any ideal realisable in all its facets. Just by studying the various forms of society, the philosophic mind becomes, he believes, free to transfer its allegiances to the realm of spirit. Santayana is concerned to follow out the analysis of society, as he comprehends it, but he is not concerned with trying to put that analysis to practical use.

JOHN W. YOLTON

*The Philosophy of Wilhelm Dilthey.* By H. A. HODGES. London: Routledge and Kegan Paul, 1952. Pp. xxvi + 368. £1 8s.

WHAT are we to do about the philosophy of Wilhelm Dilthey? We are without a translation of any of the works, but in default of the works we have an expositor. H. A. Hodges has charged himself with the task of presenting Dilthey's philosophy comprehensively to English readers. Following his hundred-page *Introduction* (1944), which was until now the only treatment in English on anything more than the scale of an article, we are now offered the full account in 360 pages which can fittingly be called closely packed, since except in the last chapter there is not a single internal heading in the book; even the chapters have numbers but no titles.

Hodges's undertaking, to present something that can be called 'the philosophy of Dilthey', is unfortunately problematic from the first, since Dilthey never succeeded in writing his philosophy down. He was an incoherent but voluminous philosophical writer. Though he possessed little power of coherent abstract thought, he was able to express himself fairly fluently in literary and historical studies. By far his most widely read work, and his most intelligible, is the volume *Das Erlebnis und die Dichtung*, consisting of four long essays on Lessing, Goethe, Novalis and Hölderlin. Published as a book in 1905 incorporating earlier material, it had run into ten editions by 1929. For his philosophy one has to go to the collected works, and there the only finished items are short occasional pieces; the rest is a very large collection of fragments varying in length from a few lines to a volume. The editors' notes are full of details about drafts, new drafts, and galley-proofs corrected and re-corrected and finally cancelled. One of the editors paints a pathetic picture of the old man looking at a cabinet containing a vast accumulation of manuscripts and knowing that he could never make a finished work out of them. His characteristic philosophical approaches were either, having called a stenographer, to begin yet another new first paragraph headed 'Life' and to

dictate a manuscript which peters out after twenty or perhaps forty pages; or to write a vast historical survey of the problem at issue, starting with the Greeks or even with "the Eastern peoples" and again petering out somewhere in recent centuries, before the conclusion that was to be the outcome of the survey has been reached. His language is loose and diffuse, his meaning often uncertain, his style the very antithesis of everything that comes from ordinary philosophical hard-thinking and considered choice of words. Hodges's book contains very frequent quotations, all rendered into English, and although mistakes of vocabulary and syntax occur in the renderings, they do scarcely any harm to the sense. For every sentence that Dilthey wrote a thousand others would have served equally well.

Out of the chaos of manuscripts the editors made something with a creditable semblance of order, though still voluminous and endlessly repetitive. Out of this in its turn Hodges has made something concise and systematic, and has even written in a chapter the *Critique of Historical Reason* which Dilthey spent his whole adult life writing for, but never managed to write. The labour involved has clearly been very great and the result is doubtless as successful as any attempt is ever likely to be; but it has only strengthened my conviction that Dilthey's philosophy was unwritten because it was unwritable as philosophy.

In the book the reader will find Dilthey's doctrines classified systematically, expounded in a concise, simple, but drab manner, and easy to locate through a full analytical table of contents, which is an excellent part of the book, and a large subject-index. After an opening chapter on Dilthey's philosophical sources, Hodges gives an account of his broader philosophical principles in two chapters, one on his theory of knowledge and one on his general theory of values. In the first he describes Dilthey's peculiar 'descriptive' form of neo-Kantianism and his attempts to establish the concepts of 'lived experience' (*Erlebnis*) and of 'structural system' in mental life. He then shows how values were for Dilthey empirically based on this lived experience (not *a priori* in character) and studied in history and the human studies, and consequently how metaphysics became for him the comparative study of conceptions of the world (*Weltanschauungen*), which he classified into three types. (The relevant volume of the collected works, together with vol. i, has been translated into French and uses the term 'conceptions du monde' which we could well borrow in order to avoid the cumbersome repetition of the German term which has become the practice in English.) In the subsequent chapters Hodges goes on to the detailed working-out of these principles in Dilthey's ethics and aesthetics, his doctrines of 'understanding' and 'meaning', his theories of method in the human studies and of the role of psychology, and his argument with Windelband and Rickert. The *Critique* is reconstructed and finally there is a point by point comparison with Collingwood. There is no bibliography, but Hodges had already provided one in his earlier book. (All this is notably free from printer's errors. The only fault I have found is the reference on page 312, which should read VIII. 222.)

My only important criticism of the book is that in separating out the philosophical elements in Dilthey's writings and detaching them from his historical and literary studies, Hodges has taken the life out of them. In those studies one can see concretely what Dilthey was trying to philosophise about. One sees how, for instance, in the first version of the *Novalis essay* in 1865, instead of treating Novalis's writings in the then usual

way as unintelligible manifestations of romantic disorder, he was able to interpret the writings and the biographical data as an *understandable* whole, and to express this interpretation for the first time in Germany. One can then see concretely that the obscure philosophical theories about understanding were attempts to cope with certain kinds of experiences and achievements in literary interpretation; and perhaps even more important, from the limitations and weaknesses of the literary study one can see the limitations of the philosophical theory. Hodges has found room in his book to mention the philosophical doctrines that Dilthey took from Novalis, but not to describe Dilthey's exhilarating experience of understanding Novalis.

One sees again how Dilthey is able to write an engrossing short biography of Lessing, incorporating an account of his aesthetic, dramatic and theological writings in a unified narrative and bringing it to a climax in a vivid presentation of Lessing's conception of the world; and one can then see concretely what the philosophical theories about *Weltanschauungen* are about. The strengths and weaknesses of the biographical method are again reflected in the philosophy. One could say much the same about his Hegel and his Schleiermacher—the examples could in fact be multiplied many times. Hodges has described Dilthey's philosophical origins very carefully. He calls them "the sources of his inspiration", but Dilthey was surely not philosophically *inspired*. His trouble was that his philosophical heritage was inadequate to deal with the kind of experience that was the basis of his philosophy, and his own creative powers as a philosopher not strong enough to enable him to invent what he needed. Hodges has not said much about Dilthey's position in the history of historiography and the history of literary appreciation. If he had said more (and it may well have been impossible inside a comprehensive account of his theories that keeps to reasonable dimensions) he would have come nearer to the sources of his inspiration. Dilthey's philosophy and philosophical connexions are not viable by themselves.

Dilthey tried to philosophise about 'the human studies', especially about 'history of ideas', but he did not succeed in discovering what there is to say philosophically about them. His unfinishable drafts and his barren acrimonious controversies are a dreadful example of what can happen when methodology ensnares a man. His work as a whole or rather in small cross-sections, taking the historical and philosophical pieces together, is stimulating as well as exasperating and full of obscurely illuminating suggestions. What we are to do about Dilthey is, I think, to read him if we can stomach him, and try again to discover what it is that there could be to say philosophically on the basis of experiences in history, the social sciences, and literature and the other arts. In the meantime we have a most valuable guide through his works in this excellent and readily usable catalogue of the elements of his abortive philosophical thought.

P. G. LUCAS.

*The Scientific Adventure.* Essays in the History and Philosophy of Science. By HERBERT DINGLE. London: Sir Isaac Pitman and Sons Ltd., 1952. Pp. ix + 372. 30s.

THE general aim of this collection of essays, written between 1937 and 1950, is to support and illustrate Professor Dingle's theory about the



nature of science, the essentials of which are set out in Chapter 9, introducing the philosophical part of the book. We are entitled, for the price, to expect better editing. Although it contains much that is valuable the book is marred by tedious repetitions of examples and anecdotes, used to support the same conclusions, so that the point could have been argued in half the space. There could have been no objection to such editing for most of the original papers are readily accessible. On the whole Professor Dingle makes his points clearly enough although his style is not always elegant and is often irritatingly "literary".

His main thesis is well known. It involves the denial of the traditional distinction between observer and thing observed and the consequent acceptance of a form of solipsism claimed to be unobjectionable. With Mach and the Vienna Circle he regards the scientist as concerned solely with the organisation of his experiences. The subject is "I here-now" and the object my past experiences so that the passage of time creates the subject-object distinction. Reason is subjective, independent of experience and serves to organise the elementary units of experience, those "which cannot possibly be destroyed but only redistributed", probably sense-data (pp. 182-183). Relations between these units are "a contribution of the subject justifiable in so far as it enables the elementary experiences to be included in a rational system but not to be regarded as inevitably associated therewith" (p. 180). This is an analysis for "the given" is neither sense-data nor physical objects but "the whole content of consciousness". It is convenient, for certain practical purposes, to divide this content into physical objects but the scientist is obviously not interested in them. He does not state laws about apples and planets, as such, but about certain "properties" (as we used to call them) of these. He does not associate the "properties" of the apple with one another but with the comparable "properties" of the planet.

The scientist, then, does not discover facts about an external world: he correlates and systematises his experiences by means of reason—and the system he constructs, aiming ultimately at all-inclusiveness, is merely the most satisfactory way he can find of relating and so understanding his experiences. The appropriate theory of measurement is operational and a chapter is devoted to justifying this along similar lines to those adopted by Professor Bridgman.

Many difficulties are raised by this account and by Professor Dingle's view that the aims and methods of philosophy and science are the same (pp. 173, 208, etc.). It is odd, too, to find him saying that "the realists sought to explain mind in terms of matter" and objecting to logicians' accounts of scientific method as not giving a picture of the way in which important discoveries were historically made. (One who gives a commentary on the Grand National is not expected either to give us an account of the winning horse's state of mind or to jump the fences himself.) However, the space available can perhaps best be used in considering the reasons which lead Professor Dingle to accept his theory.

He regards the theory as necessary to remove "the paradoxes of modern physics", mainly curved space and particles which do not behave like any observable particle. We need no longer puzzle about these conceptions for they are unnecessary if we do not attempt to interpret our observations and measurements as giving information about the properties of things. Hence the "paradoxes" lead him to a complete revision of our account of what the scientist is doing, in whatever field. This is drastic and a step not to be taken except on the greatest provo-

cation. I shall consider briefly the provocation in astronomy and atomic physics.

The basis of Professor Dingle's astronomical worries is that different methods of measurement (*e.g.* with measuring rods and by triangulation) which gave consistent results for terrestrial distances fail to do so when the immense distances of astronomy are measured. There "we find in effect that the 'direct' method and the triangulation method no longer give equal results. (Of course, they cannot be applied in their simple forms, but processes which, according to the traditional view, are equivalent to them, show that this is what we must suppose.)" (p. 321). This, unfortunately, is not very clear and no further explanation is given. Yet it is most important to get it clear for this leads, according to Professor Dingle, to that talk about the curvature of space or the distortion of measuring rods which is "paradoxical" and to which he so strongly objects. Since such discrepancies do not occur on the galactic scale, he must be considering measurements on the cosmological scale and here it is difficult to see, without further elucidation, which methods can be regarded as equivalent, on the traditional view, to the measuring rod and triangulation methods. On the cosmological scale, since parallax cannot be used, there would seem to be no method equivalent to triangulation. But however this may be, there are other reasons, both empirical and theoretical, for giving an account of the universe in terms of curved space which do not directly support an operational view of measurement.

The "paradoxes" allegedly involved in talking of "fundamental particles" are familiar. The impossibility of determining both the position and the velocity of a given particle lead us to talk about particles with velocity or position but not both or to say, even more confusingly, that "the electron must be both a wave and a particle" (p. 245). The paradox disappears if we stop regarding these "particles" as physical objects of which we are discovering "properties" for then we do not have to regard an electron as an entity with incompatible properties. This Dingle achieves by denying that it is the business of scientists ever to discover properties of objects.

Professor Dingle's difficulties rest, it seems to me, upon a mistake which he is not alone in making and do not warrant such a wholesale revision of our account of science as he proposes. Indeed, at one point he indicates the mistake but fails to draw the appropriate conclusions. He says (p. 335), "the electron has not been observed but conceived in order to express relations between things which are observed, and it is therefore not the same kind of entity as an observed body". The mistake is to think that when we call an electron a "particle" we intend to use the word to refer to something which behaves like a billiard ball. Only then does a paradox appear and this is fatal *on any view* of the nature of science. But surely we know from the beginning that we are using "particle" in this context in an unusual sense and that our verbal description does not *alone* accurately interpret our equations and observations. The expression "particle which is both a wave and a particle" is contradictory if "particle" is used both times in the same sense but if it is not then the expression is part of our account of the difference between the two senses. We can ascribe the appearance of paradox to our inability to illustrate the equations by mechanical models. Perhaps better models can be found, perhaps not. We are no doubt incautious in our use of the word "particle" but unless we are satisfied with mere uninterpreted symbols our picture, even if it is

incomplete, does help us to understand (and not merely to popularise) the theory. But the fact that our picture is inaccurate and so can be misinterpreted does not imply that the properties we ascribe to these peculiar particles belong to *nothing* external to us. We must just avoid thinking that they belong to minute billiard balls. We must recognise that saying "we cannot measure both the position and the velocity of an electron" is not at all like saying "we cannot measure both the position and the velocity of a billiard ball" and that if this shows that the properties that we ascribe to electrons cannot belong to things like billiard balls (as we should all know) it does *not* show that the properties we ascribe to billiard balls cannot belong to things like billiard balls. It is surely perverse, then, on the basis of a conception which we know to be inadequate, by itself, radically to revise our account of what the scientist is doing when he claims to discover the properties of billiard balls—or anything else.

In general, both sets of "paradoxes" can be dealt with in the same way. It is a mistake to argue, because we cannot fully explain in ordinary language a special sense of a word, that when we use that word in its ordinary sense we do not mean by it what we then think we mean. Our models of atomic structure in terms of particles and of the universe in terms of curved space are incomplete by themselves; only when we take them in conjunction with the mathematical theories and observational data can we understand them and then the "paradoxes" disappear.

PETER ALEXANDER.

*Etudes sur le temps humain.* GEORGES POULET. Edinburgh University Press. Pp. 405. 30s.

ONE may think of time as built up of independent moments, and of existence in time as a series of independent events, or one may think of events and moments as abstractions; the concrete reality being the persistence in being of variously enduring things. If one thinks in the first way, one may become very conscious, with Descartes, of the fact that, though I exist in this instant, I may not in the next, or one may brood, with (one learns from M. Poulet) Racine, over the mystery of the divine recreation from moment to moment of what does not deserve to be recreated. If, however, one thinks in the second way, one may become more conscious of the bearing of one's past upon one's present, of the fact that one's future is being created now.

No doubt the distinction between these two ways of thinking of time is unreal, and would yield to a little philosophic analysis. But it is the sort of unreal distinction which may, in default of that analysis, lead to very different conceptions of plot in play or novel, to a different kind of imaginative writing. In this book such distinctions between different "theories of time", in this sense, are used in the criticism of French writers from Montaigne to Proust; and they do shed light. M. Poulet writes with elegance and skill. If one feels at times that he is displaying a French capacity for the precise manipulation of factitious concepts, one is also conscious of receiving new insights into different ways of imagining, for each of which a "philosophy of time" can be built up.

I. M. CROMBIE.

Received also :—

- A. Arber, *The Mind and Eye*, Cambridge University Press, 1954, pp. xi + 146, 16s.
- Aristotelis Categoriae et liber de interpretatione*, by L. Minio-Paluello, Oxford at the Clarendon Press, 1949, pp. xxiii + 96, 7s. 6d.
- P. Asveld, *La Pensée Religieuse du Jeune Hegel*, University of Louvain Publications, 1954, pp. ix + 242, 135 fr.b.
- Homage to George Berkeley*, "Hermathena", Commemorative Issue, No. LXXXII, Dublin, Trinity College (Hodges Figgis & Co. Ltd.), 1953, pp. viii + 146, 10s.
- J. H. Blackhurst, *Body-Mind and Creativity*, New York, Philosophical Library, 1954, pp. 186, \$3.00.
- O. F. Bollnow, *Deutsche Existenzphilosophie*, Bern, A. Francke Ag. Verlag, 1953, pp. 40.
- E. Cassirer, *The Philosophy of Symbolic Forms*, Vol. I: Language, Yale University Press (London: Geoffrey Cumberlege), 1954, pp. xiv + 328, £1 12s. 6d.
- E. Cassirer, *The Platonic Renaissance in England*, trans. by J. P. Pettegrove, Edinburgh, T. Nelson & Sons Ltd., 1954, pp. vii + 207, 15s.
- I. M. Copi, *Symbolic Logic*, New York, The Macmillan Company, 1954, pp. xiii + 355, \$5.00.
- M. Corsi, *Natura e Società in David Hume*, Firenze, La Nuova Italia, 1954, pp. xii + 73.
- Benedetto Croce e lo Storicismo*, by F. Olgiati, Milan, Vita E. Pensiero, 1953, pp. 397, L. 1000.
- P. R. Dample, *Philosophical Essays*, Bombay, Asia Publishing House, 1954, pp. x + 208, Rs. 7/8.
- The Encyclopédie of Diderot and D'Alembert*, ed. John Lough, Cambridge University Press, 1954, pp. xv + 226, 15s.
- A. Guzzo, *La Philosophie de Demain*, Paris, Aubier, 1953, pp. 108, 345 fr.
- Sören Halldén, *Emotive Propositions*, A Study of Value, Stockholm, Almqvist and Wiksell, 1954, pp. 232, Sw. kr. 15.
- O. Hamelin, *La théorie de l'intellect d'après Aristote et ses commentateurs*. *Heraclitus, the Cosmic Fragments*, a Critical Study with Introduction, Text and Translation by G. S. Kirk, Cambridge University Press, 1954, pp. xvi + 423, £2 10s.
- I. L. Horowitz, *Claude Helvetius, Philosopher of Democracy and Enlightenment*, New York, Paine-Whitman, 1954, pp. 196, \$3.00.
- K. N. Kar and Maung Hla Bu, *A Text Book of Modern Formal Logic*, Calcutta, The Indian Publicity Society, 1953 pp. 241.
- P. H. van Laer, *Philosophico-Scientific Problems*, trans. by H. J. Koren, Duquesne University Press (U.K.: Basil Blackwell), 1953 pp. xi + 168, £1 5s.
- V. F. Lenzen, *Causality in Natural Science*, Springfield, Ill., C. C. Thomas (U.K.: Basil Blackwell), 1954, pp. vii + 121, £1 1s. 6d.
- W. Luther, *Weltansicht und Geistesleben*, Göttingen, Vandenhoeck & Ruprecht, 1954, pp. 112, D.M. 9.80.
- R. MacCallum, *Imitation and Design*, ed. W. Blissett, University of Toronto Press (London: Geoffrey Cumberlege), 1953, pp. xvi + 209, £2.
- T. M. P. Mahadevan, *Time and the Timeless*, Principal Miller Lectures, 1953, Madras Upanishad Vihar, pp. 88, Rs. 2/-.
- A. R. M. Murray, *An Introduction to Political Philosophy*, London, Cohen & West, Ltd., 1953 pp. vii + 240, 12s. 6d.

- Leonard Nelson, *Zum Gedächtnis*, ed. M. Specht and W. Eichler, Frankfurt-a-Main, Verlag Öffentliches Leben, 1954, pp. 303, D.M. 14.80.
- G. H. R. Parkinson, *Spinoza's Theory of Knowledge*, Oxford, at the Clarendon Press, 1954, pp. x + 197, £1 1s.
- Plato's *Socratic Dialogues*, translated and edited by W. D. Woodhead, Edinburgh, T. Nelson & Sons Ltd. (Philosophical Texts), 1954, pp. xxxii + 308, 10s. 6d.
- G. Quadri, *La Vita Estetica e lo Sviluppo della Coscienza*, Firenze, La Nuova Italia Editrice, 1954, pp. 158.
- W. V. O. Quine, *From a Logical Point of View*, Harvard University Press (London: Geoffrey Cumberlege), 1954, pp. vi + 184, £1 2s. 6d.
- H. Reichenbach, *Nomological Statements and Admissible Operations*, Amsterdam, North-Holland Publishing Co., 1954, pp. 140, £1 7s.
- Relazioni e Discussioni* (1951-1952), Milan, Fratelli Bocca, 1954, pp. 168, L. 1000.
- Rousseau *Political Writings*, translated and edited by F. M. Watkins, Edinburgh, T. Nelson & Sons Ltd. (Philosophical Texts), 1954, pp. xliii + 330, 10s. 6d.
- O. Samuel, *A Foundation of Ontology*, A Critical Analysis of Nicolai Hartmann, New York, Philosophical Library, 1954, pp. xv + 155, \$3.75.
- The Philosophy of Santayana*, ed. I. Edman, London, Constable & Co. Ltd., 1953, pp. lxii + 904, £2 5s.
- K. Schilling, *Geschichte der Philosophie II*, Basel, Ernst Reinhardt Verlag, 1953, pp. 686, Fr. 32.
- M. Soreth, *Der Platonische Dialog Hippias Maior*, Munich, Verlag C. H. Beck, 1953, pp. 64, D.M. 6.50.
- S. Strasser, *Le problème de l'Âme*, trans. J.-P. Wurtz, Louvain University Publications, 1954, pp. xiv + 257, 180 fr.b.
- P. Tillich, *Love, Power and Justice*, Ontological Analyses and Ethical Complication, Oxford University Press (Geoffrey Cumberlege), 1954, pp. viii + 125, 10s. 6d.
- Thomas De Vio, Cardinal Cajetan, *The Analogy of Names and the Concept of Being*, trans. by E. A. Bushinski and H. J. Koren, Duquesne University Press (U.K.: Basil Blackwell), pp. x + 93, 18s. 6d.
- H. Wagner, *Existenz, Analogie und Dialektik*, Basel, Ernst Reinhardt Verlag, 1953, pp. 226, Fr. 19.
- Jean Wahl, *Les Philosophies de l'Existence*, Paris, Librairie Armand Colin, 1954, pp. 175, 250 fr.
- A. N. Whitehead, *An Anthology*, selected by F. S. C. Northrop and Mason W. Gross, Cambridge University Press, 1954, pp. 928, £3 15s.
- 
- J. Ehrenwald, *New Dimensions of Deep Analysis*, London, George Allen & Unwin Ltd., 1954, pp. 316, £1 5s.
- R. Garnier, *Cours de Cinématique*, Paris, Gauthier-Villars, 1954, pp. 244, 4000 fr.
- J. S. Gray, *Psychology applied to Human Affairs*, London, McGraw-Hill Publishing Co. Ltd., 1954, pp. vii + 581, £2 2s. 6d.
- A. Huxley, *The Doors of Perception*, London, Chatto & Windus, 1954, pp. 63, 6s.

- H. C. Lehman, *Age and Achievement*, Princeton University Press (for the American Philosophical Society) (London : Geoffrey Cumberlege), 1953, pp. xi + 359, £3.
- E. Minkowski, *La Schizophrénie Psychopathologie des Schizoïdes et des Schizophrènes*, Paris, Desclée de Brouwer, 1954, pp. 254, 150 fr.b.
- J. Nuttin, *Psycho-Analysis and Personality*, A Dynamic Theory of Normal Personality, trans. G. Lamb, London, Sheed & Ward, 1954, pp. xiv + 310, 16s.
- J. Nuttin, *Tâche Réussite et Echec Théorie de la conduite humaine*, Louvain, University Publications, 1953, pp. ix + 530, 280 fr.b.
- L. F. G. de Onrubia, *Psicologia Intencional*, University of Buenos Aires Institute of Philosophy, 1953, pp. 73.
- L. Peck, *Child Psychology*, Boston, D. C. Heath & Co., 1954, pp. 536.
- L. M. Ravagnan, *La Unidad Psicofísica*, University of Buenos Aires Institute of Philosophy, 1952, pp. 38.
- A. M. Weitzenhoffer, *Hypnotism*, An objective study in suggestibility, New York, John Wiley & Sons Inc. (London : Chapman & Hall Ltd.), 1954, pp. xvi + 380, £2 8s.
- 
- American Scholarship in the Twentieth Century*, ed. Merle Curti, Harvard University Press (London : Geoffrey Cumberlege), 1953, pp. vii + 252, £1 16s.
- A. C. Bouquet, *Sacred Books of the World*, A Pelican Book, 1954, pp. 343, 3s. 6d.
- A. Denjoy, *L'Énumération Transfinie*, III, Études Complémentaires sur l'ordination, IV, Notes sur les sujets controversés, Paris, Gauthier-Villars, 1954.
- Galileo Galilei, *Dialogue concerning the Two Chief World Systems*, trans. by S. Drake, University of California Press (London : Cambridge University Press), 1954, pp. xxvii + 491, £3 15s.
- A. Hamer Hall, *The Fundamentals of World Peace*, New York, Philosophical Library, 1953, pp. 112, \$3.00.
- A. H. Kamiat, *The Ethics of Civilisation*, Washington, D.C., Public Affairs Press, 1954, pp. vi + 80, \$2.00.
- U. Karmi, *Inspiration Divine*, Paris, Editions Vitiano, 1954, pp. 123, 950 fr.
- L. Kochan, *Russia and the Weimar Republic*, Cambridge, Bowes & Bowes, 1954, pp. x + 190, £1 5s.
- J. Lindberg, *Foundations of Social Survival*, Columbia University Press (London : Geoffrey Cumberlege), 1954, pp. viii + 260, £1 8s.
- R. Neutra, *Survival through Design*, Oxford University Press, 1954, pp. ix + 384, £1 16s.
- Newton's *Philosophy of Nature*, ed. by H. S. Thayer, Introduction by J. H. Randall, Jr., New York and London, Hafner Publishing Co., 1953, pp. xvi + 207, 18s. 6d.
- W. J. Trjitzinsky, *Les problèmes de totalisation se rattachant aux lapluciens non sommables*, Paris, Gauthier-Villars, 1954, 1400 fr.
- E. J. Trueblood, *The Dawn of the Post-Modern Era*, New York, Philosophical Library, 1954, pp. xii + 198, \$3.75.

- Uncertainty and Business Decisions*, a Symposium by W. B. Gallie, D. J. O'Connor, I. J. Good, G. P. Meredith, C. F. Carter, B. R. Williams and A. D. Roy, Liverpool University Press, 1954, pp. viii + 104, 10s. 6d.
- E. Wood, *Great Systems of Yoga*, New York, Philosophical Library, 1954, pp. xviii + 168, \$3.50.

- Diogenes*, quarterly publication of the International Council for Philosophy and Humanistic Studies, edited by Roger Caillois, No. 3, 1953, 21s. per annum.
- Kant-Studien*, Philosophische Zeitschrift Begründet von Hans Vaihinger, ed. G. Martin, Köln, Kölner Universitätsverlag, 1953-54, pp. 320, Annual subscription, D.M. 28.
- L'Industria*, Rivista di economia politica, ed. F. di Fenizio, No. 4, 1953.
- Journal of Psychotherapy as a Religious Process*, Dayton, Ohio, Institute for Rankian Psychoanalysis, 1954, vol. i, pp. 108, \$2.00 per annum.
- Ora & Labora*, Revista Liturgica Beneditina, ed. Mosteiro de Singeverga, Ano 1, 1954, pp. 64.
- Wisdom*, ed. John Armido, No. 1, London, The Rosery Press, 1954, pp. 27, £1 1s. per annum.

## IX.—NOTES

THE Editor will be out of England from early August to early November. Editorial correspondence, decisions, etc., will be in abeyance during that period.

## MIND ASSOCIATION

Those who wish to join the Association should communicate with the Hon. Treasurer, Mr. J. D. MABBOTT, St. John's College, Oxford, to whom the yearly subscription of sixteen shillings (payable in advance) should be sent. Cheques should be made payable to the Mind Association. Members may pay a Life Composition of £21 instead of the annual subscription. The annual subscription may be paid by Banker's Order; forms for this purpose can be obtained from the Hon. Treasurer.

In return for their subscriptions members receive MIND gratis and post free, and (if of 3 years' standing) are entitled to buy back numbers of both the Old and the New Series at half-price, if still in stock.

The Hon. Secretary of the Association is Professor KARL BRITTON, Dep. of Philosophy, King's College, Newcastle-on-Tyne.

Members resident in U.S.A. may pay the subscription (\$2.80) to the Hon. Assistant-Treasurer, Professor B. Blanshard, Dept. of Phil., Yale University, New Haven, Conn.

Those who join the Association after previously buying MIND direct from the publishers or through a bookseller are asked to inform the Treasurer of this (and of the name of the bookseller) when they join.